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ABSTRACT

In examining the literature dealing with the information gathering and disseminating behavior of scientists, this review focuses on use studies to identify objectives and methods shared by investigators in the field, two studies deemed to be milestones on the road toward understanding scientific information flow, network studies which are concerned with interrelationships among communication artifacts without reference to the behavior of individual scientists, and, finally, the flow of scientific information to the public. (Author/SP)

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THE FLOW OF (BEHAVIORAL) SCIENCE INFORMATION

A REVIEW OF THE RESEARCH LITERATURE

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Note on the Third Printing

Various people, finding this review useful, have suggested a revision encompassing the many good studies of scientific information flow that have appeared in the past year. The decision not to revise is based upon the scope and quality of the "Information Needs and Uses" chapter (by Herbert Menzel) in the first Annual Review of Information Science and Technology (Carlos Cuadra, editor, New York: John Wiley and Sons, 1966). Henceforth the function served by ad hoc reviews is likely to be served systematically, on a yearly basis, by the Annual Review.

Another printing is necessary, however, because of a backlog of unfilled requests. Together with earlier reviews, this survey of work completed before 1966 may still serve a catching-up function, orienting the reader to a lively and important research area.

WJP

December 1, 1966

FOREWORD

This review was made possible by support from the National Academy of Sciences - National Research Council Committee on Information Processing in the Behavioral Sciences (through Wilbur Schramm's subcommittee) and from the National Science Foundation Office of Science Information Service (through Grant #GN-434 to Edwin B. Parker, with whom the reviewer is working on the project, "Science Information Exchange among Communication Researchers"). The review is intended to serve the subcommittee as a working paper on the current state of scientific information flow research. It will also serve as the initial (general) literature review of the NSF project.

CONTENTS

CONTENTS (continued)

CONTENTS (continued)

CONTENTS (continued)

CONTENTS (continued)

CONTENTS (continued)

THE FLOW OF (BEHAVIORAL) SCIENCE INFORMATION
A REVIEW OF THE RESEARCH LITERATURE[1]

I

INTRODUCTION

If such a literature existed, this review would confine itself to studies of the flow of behavioral science information. With a few exceptions (notably the American Psychological Association's Project on Scientific Information Exchange in Psychology), that literature does not yet exist. Behavioral scientists have been considered parenthetically, at best, in research on scientific information flow. A review that attempts to represent the flow of behavioral science information must cull studies of promising generality from the large, diffuse, sometimes poorly executed and duplicative body of research focusing on physicists, chemists, zoologists, engineers, etc. As it happens, information-gathering and -disseminating behavior of scientists does not seem to be affected greatly by their specific fields of research, whereas other factors in their research environments and in their professional backgrounds do seem to be influential. We shall infer that information flows to and from behavioral scientists in much the same way that it flows to and from physical scientists until we have better data on the former group.

What this review will cover. Any study dealing with the information-gathering and -disseminating behavior of scientists has been considered relevant to this review. Most often such studies report the daily information-processing activities of a group of scientists circumscribed by field, by membership in an association, by employment in a laboratory, etc. A few studies focusing only on communication artifacts (e.g., journal articles, convention presentations) have been included for their value in sketching the natural history of a scientific communication process (e.g., bibliographic coupling among journal articles, the publication fate of convention presentations).

What this review will not cover. Much of the literature dealing with scientific information will not be mentioned in this review for various reasons. Specifically excluded:

(1) Technical aspects of information-retrieval (e.g., indexing systems, machine storage, automatic abstracting).

(2) Information-system policy recommendations, suggestions for improving information flow.

(3) Studies showing only that the rate of publication or of any other form of communication is increasing (the usual verb is "exploding") throughout a field.

(4) Most of the citation-counting literature, in particular the many Master's theses with the generic title, "Characteristics of the Literature Used by Authors of Journal Articles in the Field of ____________." References listed at the end of a journal

article tell us little about the information inputs that actually shaped the research being reported.

(5) Most studies showing patterns of use of a single channel or source without providing comparative data on use of other channels or sources. Usually the sampling and data collection procedures in such studies are idiosyncratic; they differ from procedures in other studies reporting parallel data.

(6) Studies, conducted at a library or information center, in which call slips are analyzed to determine what information is in greatest demand. At best we cannot relate these demands to scientists' other information-seeking efforts. At worst, in some cases, there is no evidence that the users are even scientists.

Guides to the literature of research on scientific information flow. Except for studies of technical aspects of information storage and retrieval, descriptions of most of the literature on scientific information flow may be found in one or more of these sources:

(1) Törnudd, 1959. An easily obtained summary of information-use studies published before 1958. Törnudd drew many of her abstracts of pre-1954 work from Shaw (1956). She made no attempt to criticize faulty methodology or to distinguish good studies from bad.

(2) Menzel, 1960. A review of studies for the National Science Foundation. Menzel attempted to collate tables of findings from more or less comparable studies; footnotes mark the limits of comparability. Comparison is limited to measures of use; scientists' evaluation of sources and statements of satisfaction are systematically excluded as

"of little usefulness to the planning of action of more than local scope", except when they serve to interpret use patterns. Twenty-six pre-1960 studies are included, although some contribute to only one or two tables. Menzel's discussion of shortcomings in methodology and analysis is excellent.

(3) Davis and Bailey, 1964. An annotated bibliography of 438 "use studies", including virtually every study of significance up to 1963. This is a difficult source to use because of the high proportion of chaff; about 350 of the citations are commercial periodical readership studies and library school research exercises. Annotations are sometimes incautious: in describing the Thorne study (1954), in which the response rate was a dismal 30.3 per cent, the annotator reports that the "average member of the (R.A.E.) staff spent 5.1 hours per week reading scientific literature." A self-selected minority of a sample does not provide data on the average member.

(4) National Science Foundation, Office of Science Information Service, 1965. Recently published or yet-unpublished work is reported in Current Research and Development in Scientific Documentation, No. 14. These are brief progress reports describing objectives and methodology. Findings are reported in minimum detail, and in many instances data have not yet been collected.

The organization of this review. Part II of this review presents an overview of "use studies" and an eclectic chronology of such studies. It is the purpose of this section to identify objectives and methods shared by investigators in the field. Findings will be mentioned but not stressed, for reasons elaborated under the heading, "Problems of Interpretation".

Part III presents detailed summaries of two studies deemed by the reviewer to be milestones on the road toward understanding scientific information flow. To labor the metaphor, these are undoubtedly not the only milestones, and the end of the road is not in sight beyond them, but the two longer summaries of what was done and what was learned provide a frame of reference for viewing other efforts.

Part IV describes a few studies focusing on communication artifacts (e.g., journal articles, convention presentations). Such "network studies" are fundamentally different from use studies; they are concerned with interrelationships among the artifacts without reference to the behavior of individual scientists. Their perspective is systemic, in the tradition of the quantitative study of the history of science (cf. Price, 1963).

Part V reviews the literature on a corollary and most inadequately researched topic, the flow of scientific information to the public.

II

USE STUDIES, 1948-1965

An Overview of Use Studies

Studies of scientists' information-processing activities have become known generically as use studies. This label betrays the bias of documentalists who have conducted use studies -- the scientist is a user of their information commodity. It also forewarns the reader that the scope of a use study is likely to be restricted to the use of a single information source, or to the use of several information sources by a single target group of scientists.

The great majority of use studies are mutually duplicative. Essentially the same study has been conducted independently with physicists, chemists, forest service technologists, engineers, physiologists, biochemists, zoologists, and so on and on. If each successive study had replicated the methods of its forerunners, changing only the sample of scientists, we would now have an impressive pool of data. Since no noticeable effort has been made to replicate methods, we now have a collection of case studies whose findings can be compared only if a <u>ceteris paribus</u> assumption is stretched over gross differences in procedure.

<u>Why have so many use studies been conducted</u>? Without exception (to this reviewer's knowledge), use studies have been conducted <u>to guide information policy</u>. This is most clearly evident in studies conducted by scientific associations, by corporations, by government

agencies, and by consulting documentalists. Behind other, apparently unaffiliated studies stand the National Science Foundation (particularly the Office of Science Information Service), the Air Force Office of Scientific Research, the National Institutes of Health, the Department of Scientific and Industrial Research (in the U.K.), the Association of Special Libraries and Information Bureaux (in the U.K.), and many other sponsoring agencies.

There seem to be two primary reasons why <u>so many</u> use studies have been conducted. The first is a (well-founded) distrust of the findings of earlier studies. The second is a conviction that scientists in <u>this</u> discipline, or in <u>this</u> association, or in <u>this</u> agency, are so unique in their information-processing behavior that only a new study will suffice to guide information policy.

Superficially the hypothesis of uniqueness is confirmed each time. Although the broad outline of information flow changes little from study to study, there are differences that would lead to different policy recommendations <u>if</u> they could be taken as reliable and valid. Given the divergent methodologies of these studies, however, such differences may be artifactual.

<u>Use study methodologies</u>. In a use study one collects data on use, with the help of questionnaires, interviews, diaries, request logs, participant-observers, etc. The fact that each of these methods has been applied to the information-flow problem should be a healthy sign -- independent measurement of the same phenomenon with different instruments enhances validity, just as repeated measurement

with the same instrument enhances reliability. Yet each method has inherent reliability and validity defects seldom examined by investigators in this field. Moreover, the phenomenon under measurement has not been held constant, with the result that changes in the sample are confounded with changes in method.

It is important to an interpretation of information-flow data to consider the strengths and weaknesses of each method:

(1) Questionnaires. In almost every instance in which the sample of scientists has been geographically scattered, mail questionnaires have been used to sample their information-processing behavior. One of the earliest questionnaire studies was Bernal's (1948), and among the most recently published questionnaire studies are the APA series (Garvey and Griffith, 1963-). Other questionnaire studies are now in progress.

The use of questionnaires to overcome geographical distance is usually a necessary methodological compromise. The cost of interviewing or participant-observation with a geographically scattered sample is prohibitive. Only when the scientists, although spread across the country, are concentrated in a few laboratories (as in Sieber, 1964) can a centrally trained and supervised staff of traveling interviewers be used.

It seems not at all defensible, however, to distribute a questionnaire to scientists in a single institution (cf. Thorne, 1954), particularly if a low response rate is tolerated. To state this reviewer's bias explicitly, the questionnaire is a second-best method to be employed when personal interviews are for some reason not practical.

There are four counts against the use of questionnaires to collect data on information flow: (1) the response rate is likely to be unacceptably low (30 per cent in Thorne's study, even lower in some studies omitted from this review), (2) we have no way of knowing the respondent's state of mind (e.g., serious, jesting) when the questionnaire is being completed, (3) we have no way of knowing that he understands the questions, (4) opportunities to probe an incomplete response are very limited.

If the investigator does not settle for a low response rate (since the subsample of responders cannot be assumed to represent the sampled population unless the response rate approaches 100 per cent), and if respondents take the questionnaire seriously, and if individual pretesting provides assurance that instructions are understood and questions uniformly interpreted, and if probes are inserted to limit the frequency of partial responses, then the validity problems of the questionnaire technique do not overbalance its obvious practical advantages.

(2) Diaries. In several studies respondents have been asked to keep diary records of their information-processing behavior. An early example is Bernal (1948), who used both a diary and a questionnaire. A recent example is the first study in the APA series (Garvey and Griffith, 1963-). In such usage the diary is a serialized questionnaire, differing from the usual questionnaire in its attention

to ongoing rather than past or "typical" behavior. The four primary threats to questionnaire validity apply equally to the diary technique. It is a recognized problem with diaries, moreover, that busy respondents will not keep a running record of each new information-processing act but instead will try to catch up periodically by recalling their activities during the elapsed time. Nor is the diary technique well adapted to information that does not come in "packages" -- identifiable books or articles, convention presentations, colloquia, etc. Few diarists could keep track of all the "unpackaged" information they receive, for instance, in corridor conversations at scientific meetings. Lacking uniform definitions of "unit of information" and "information-processing act", it is also difficult to compare diary entries of one scientist with those of another scientist.

With adequate instructions, and with some means of motivating respondents to keep the record up to date, the diary may be preferred over the usual questionnaire if ongoing behavior is more pertinent to the objectives of the study than past or "typical" behavior.

(3) Interviews. The personal interview is potentially a highly valid technique for collecting data on scientific information flow. Because an interviewer can make repeated call-backs, the response rate of an interview study usually exceeds that of a questionnaire study. The interviewer can also gauge the respondent's mood (serious or jesting), determine whether he understands the questions, and probe as necessary for response detail. At best the validity of an interview study is limited only by the respondent's ability to

recall his behavior and by his willingness to report it without embellishment. At worst there may be interpersonal difficulties, a heightened sense of "guinea-piggism", and simple inability to answer ill-conceived questions (such as one that continues to appear in various forms, "About what per cent of useful information on __________ do you obtain from journals, books, abstracts, convention presentations....?").

Herner (1954) and Menzel (1958) are two often-cited examples of interview studies in this field.

(4) Participant-observers. If the investigator wishes only to learn how much time the scientist spends with each information source, then the use of a participant-observer has much to commend it. Unlike the scientist himself, the participant-observer does not forget to record an information-processing act just when it becomes most engrossing. Unlike the scientist, the participant-observer has no reason to embellish the record (e.g., to show high use of a prestigious information source). Since participant-observers can be trained collectively in record-keeping procedures, it is more justifiable to compare their records across several scientists than to compare self-kept records of the same scientists.

These advantages notwithstanding, participant-observation is a severely limited technique. The observer can collect data only when the scientist is within his range of observation. As applied thus far (Ackoff and Halbert, 1958), the range of observation has been restricted to the scientist's laboratory. This is one of the few locales in which the observer may unobtrusively keep track of the scientist's activities. The other possible locales -- in the library, at scientific meetings, etc. -- by no means exhaust the settings in which scientific information-processing takes place. The scientist is quite likely to work at home, where no one has yet suggested that his behavior be observed. He is likely to read on airplanes, where the observer's surveillance is possible but costly.

If the investigator wishes to know something about the purposes of information-seeking, about attitudes toward information sources, about content of information regarded as significant, etc., then participant-observation must be combined with another technique that permits introspective response.

Reliability of the four methods. The reliability of all brief-observation methods (questionnaires, diaries, interviews, short-term participant-observation) is about equal. Respondents have often complained that the period of time sampled was not typical in the ways they were occupied and in the ways they used information (cf. APA-PSIEP #1, Garvey and Griffith, 1963-). In order to infer that his data are representative of any other sample of time, the investigator must assume that his chosen sample of time was no more atypical for the group of scientists than other times. If the scientists are all working on the same project (as might be the case when all are employed by the same corporation), their information-processing activity may primarily reflect the phase they have reached on that project. If all are members of the same association, they may be attending a meeting during the sampled time. Unless the sample is quite heterogeneous on attributes associated with the time sample, observations made in one time period, although valid for that period, may prove to be unreliable under the test of repeated measurement. Repeated measurement is, of course, the key to reliability.

It would seem that questionnaires and interview schedules that stress typical or habitual behavior are immune to time-sample contamination. This is true only to a degree, since any man's memory is to some extent the captive of recent experiences. A recent period of daily library research may lead a scientist to report that he habitually uses the library, which may not be the case.

Problems of interpretation. In the chronology that follows, no systematic attempt is made to compare findings from two or more studies. There are two reasons why such comparison might be made: (1) The fact that sample A is high on behavior X while sample B is low shows that X does vary and may deserve further study. (2) The fact that sample A is high on behavior X while sample B is low may be associated with intersample differences on attribute Y; if so, the X-Y relationship can be studied further to account for as much X variation as possible.

Reason (1), proof of variation, is trivial. The one generalization these studies strongly support is that all information-processing behaviors vary -- from country to country, sample to sample, subgroup to subgroup, person to person.

Reason (2), proof of covariation, would justify interstudy comparisons if such proof seemed attainable. Even within the same study, apparent correlation between behavior X and attribute Y may be spurious -- that is, explained by the correlation of both X and Y with a third variable Z. The counterexplanation of Z's effect can be tested by partialling if the investigator has thought to collect data on Z. In a nonexperimental design, however, there frequently remain other variables, measured and unmeasured, that may explain away the X-Y relationship.

When findings are compared between studies to show that (for instance) British scientists are primarily dependent on print sources while American scientists are primarily dependent on interpersonal sources, there are no adequate tests of a host of counterexplanations. We may expect that the British and American studies differ in design, in sponsorship, in objectives, in sampling procedure, in data-collection method, in response rate, in the immediate context (or "set") of relevant questions, and in an entire range of scientist attributes such as professional background, area of specialization, institutional affilation, rank, functions, and so on and on. Some of these factors (e.g., area of specialization) are controllable in the analysis; others (e.g., data-collection methods) are not. The hypothesis that nationality is correlated with information-source dependence cannot be regarded seriously until all the obvious counterexplanation have been dealt with. Note that the issue of causation has not been introduced at all. Interstudy comparisons in this field fail even to identify nonspurious correlations.

It should be less difficult to identify nonspurious correlations by comparing subgroups within the same study. The investigator can collect data to close off many counterexplanations of the relationships he is studying. Other confounding factors are eliminated by the design itself: a uniform sampling procedure, a standardized data-collection instrument, and consistently high response rates within subgroups rule out three sources of spuriousness.

Yet few of these studies have been analyzed as powerfully as their data permit, and the reviewer can say little about findings that have not been subjected to routine tests of spuriousness. For example, Bernal (1948) found that scientists in various fields read different numbers of journal articles per week (e.g., biochemists read the greatest number, engineers the least). He found that scientists of higher and lower rank also differed in this behavior (e.g., professors and directors read the greatest number, lecturers and assistant directors the least).

Since Bernal's sample contained different proportions of scientists _by_ rank _within_ field, neither finding can be accepted as nonspurious on the face of it. Did the biochemists rank first in reading behavior because they had more than their share of professors and directors? Or did the professors and directors rank first because they had more than their share of biochemists? The required table, reading behavior _by_ rank _within_ each field, is not presented.

Fortunately, studies have been improving in this respect. Three-way and four-way tabulations now appear. When information-source use is tabulated _by_ purpose of the information _by_ the scientist's area of specialization _by_ his institutional rank, something can be done with the findings.

An Eclectic Chronology of Use Studies, 1948-1965.

Information-flow research published before 1948 relied upon reference-counting methods to identify most-used books and periodicals. Such research focused on communication artifacts, not on scientists' behavior in itself. The Royal Society Scientific Information Conference of 1948 gave impetus to the study of scientists' information-gathering and -disseminating behavior, and this chronology begins with two studies reported at that conference.

Many studies are mentioned below only in passing. In the reviewer's opinion, these studies stand somewhat in the shadow of contemporary studies reporting sounder methodology and richer data. Since space does not permit a full summary of any study, the reader should consult original reports of studies that interest him, whether summarized here or not.

Bernal, 1948. Using a combination of questionnaire and diary, Bernal collected data from 208 British scientists at 8 government, university, and private research institutes. Scientific fields included geology, physics, mathematics, chemistry, biochemistry, biology, and engineering. Bernal does not state the size of his initial sample, and therefore a response rate cannot be computed, but he acknowledges that "the sample was biased, as only those willing to take the trouble, and consequently people more careful than the others, took part in it."

There were two forms of diary cards, one for casual perusal of the literature and one for specific journal searches. The questionnaire established the respondent's position, institution, and field of specialization, as well as his use of abstracts, reviews, and reprints.

The format of Bernal's diary cards and questionnaire constrained the range of behaviors that scientists could report. Questions focused narrowly on the serial literature. No comparative data were collected on scientists' use of books, convention presentations, symposia, casual conversations, etc. Respondents had no opportunity to report happenstance encounters with print sources or with other people in which valuable information was gained. As a result, they seem to be remarkably methodical gatherers of information.

Discrepancy between journal subscription and journal use provided the only hint of irrationality. The Journal of the American Chemical Society, to which only two respondents subscribed, was consulted 139 times by 34 persons during the course of the survey. The Journal of Biological Chemistry, to which no respondent subscribed, was consulted 74 times by 22 persons. At the other end of the range, British Abstracts, to which 19 respondents subscribed, was consulted only 12 times by 5 persons.

In spite of its small compass and ambiguous data, this study was important as a precedent for more satisfactory efforts of the early 1950's (such as Herner, 1954, conducted in 1952) in which Bernal's mistakes are cited as instructive negative examples.

Urquhart, 1948. Users of the London Science Museum Library were surveyed to determine what references led them to request publications, whether the publications proved to contain needed information, and for what purposes information was being sought. The 354 returned questionnaires represent a response rate of 49.5 per cent, with almost half the responses coming from engineers and applied chemists (other fields significantly represented were physics, chemistry, biology). The Science Museum Library serves scientific organizations rather than individual citizens, and there is some assurance that these respondents were all scientists or at least technologists.

Urquhart's report contains some intriguing cross-tabulations. He anticipated Menzel's functional analysis (1958) by asking respondents whether the information was needed for "theoretical research", "experimental details", "experimental results", "technical development work", or for "general information". Tabulating these uses against date of publication, he found that recent publications were most used for "technical development work", whereas older publications were consulted for "experimental details".

Tabulating use against source of reference, Urquhart did not find, as did Menzel (1958), that colleagues especially provided references to articles of methodological interest (in the absence of adequate indexing of methodologies and procedures). Other people did provide references proportionately more often than abstracts to literature for "theoretical research" and "technical development work" uses, whereas abstracts provided proportionately more references for "experimental details" and "experimental results" uses.

A breakdown of "reference misses", in which the suggested publication did not contain needed information, shows that verbal recommendations yielded fewer misses than did abstracts, but citations from other articles were even more on target.

It should be noted that Urquhart's data tell us nothing about the users except their fields of science. We do not know their professional backgrounds, nor their research environments, nor their rank relative to other scientists, nor their use of other information sources. Without such additional data on the users we cannot begin to interpret these findings on journal use.

It is clear from Urquhart's subsequent work (1959) that he is interested in information flow in terms of literature networks and systems, not in terms of individual behavior. A stronger contrast could not be made between Menzel's analysis of the behavior of 77 scientists (1958) and Urquhart's analysis of 87,255 information-seeking acts (1959), which will be considered in Part IV of this review.

Johns Hopkins University, Welch Medical Library, 1950. Interviews were conducted with medical scientists and librarians. Emphasis was on use of bibliographies, abstracts, indexes.

Scates and Yeomans, 1950. Scientists working in naval shipyards and in industrial firms were surveyed by questionnaire. Previously a group of scientists in the Bureau of Ordnance had been interviewed to provide information for constructing the questionnaire. Different versions of the questionnaire were used in the various organizations, reducing data comparability. A curious conceptual distinction was made between use of the literature and "self-educational activities" (such as attendance at scientific meetings).

Törnudd, 1953. Chemists at the Mellon Institute were studied via a questionnaire dealing with reading habits, felt needs, and opinions of the usefulness of sources. The response rate of 95.6 per cent is extraordinarily high for a self-administered questionnaire.

Herner, 1954. This study of scientists affiliated with Johns Hopkins University, conducted in 1952, does not show its age. It can be faulted (e.g., important cross-tabulations are omitted), but in the behaviors it covers and in its data-collection procedure it was well thought-out and well executed.

Interviews were obtained from 606 pure and applied scientists in physics, chemistry, biology, mathematics, earth science, medicine, psychology, and engineering. Half the sample had doctoral degrees and an additional 21 per cent had Master's degrees. The sample ranged in age from 21 to 93, with a median age of 35.

Interviewers were provided with a fully structured interview schedule. The interviewer's responsibility was mainly that of interpreting ambiguous questions and probing incomplete responses. Herner's discussion of the reasons that led him to prefer personal interviews over self-administered questionnaires is one of the first reviews of what has become a chronic methodological problem of this field.

No mention is made of the initial sample from which the 606 completed interviews came. This is unfortunate, since we do not know to what extent the 606 are a self-selected group. The sampling procedure as described suggests a quota sample rather than a probability sample. If so, no record may have been kept of uncooperative scientists, although the size and composition of this group is certainly of interest.

Some of Herner's findings:

(1) The only factors definitely related to information-gathering habits of the interviewed scientists were their fields of work, the type of scientific organization in which they were working, and whether they were working in pure or applied science. Extent of formal education and age -- at least between ages 21 and 50 -- did not appear to be related to information-gathering habits.

(2) In response to the question, "Is required technical information obtained mainly from conversations and conferences or scientific literature?", scientists in various fields and in pure and applied research expressed greater dependence on the literature over-all. In response to the question of what percentage of "technical information" is obtained from conferences, conversations, and the literature, these median percentages of dependence on the literature were obtained:

	Pure	Applied
Chemistry	75%	60%
Mathematics	80	50
Physics	75	50
Biology	60	-
Earth Sciences	85	-
Engineering	-	60
Medicine	-	40

Such percentages are suspect, however, on the grounds that not even a scientist can perform the necessary mental arithmetic.

(3) Pure and applied scientists agreed within two percentage points in their relative use of advanced textbooks and monographs, elementary textbooks, handbooks, dictionaries and glossaries, encyclopedias, tables, theses, patents, and supply catalogs. Pure scientists were more than two percentage points higher than applied scientists in their relative use of research journals and review publications, while the converse was true of trade publications, research reports, and specifications and standards. In aggregate ranking research reports head the list, followed by advanced textbooks and monographs, research journals and handbooks (tied), tables, and elementary textbooks and review publications (tied).

(4) Herner's data support Törnudd's finding (1953) that reprints are used more and appreciated more by pure than by applied scientists.

(5) Data are skimpy on the information-gathering habits of the 25 psychologists, the only behavioral scientists in the sample. It is reported that their preferred print sources are research journals, review publications, dictionaries and glossaries, and classified

and unclassified research reports, but the order of preference in not stated. We also learn that the psychologists lead all groups in annual purchase of books and subscription to journals. Otherwise data on the psychologists are summed with those of other <u>applied</u> scientists.

(6) Herner compared engineers in the School of Engineering with engineers in the Applied Physics Laboratory "to see whether the types of <u>direct</u> [i.e., print] sources of information that a scientist uses are a function of the type of organization in which he works." The two groups differ strikingly in their first five choices; only advanced textbooks and monographs (as one category) and research journals appear on both lists. Of course we cannot be confident that the two groups are otherwise similar (e.g., they may differ in professional background, in areas of research interest, etc.), but Herner has shown that scientists nominally in the same professional group may differ systematically in information-source preferences.

(7) Pure and applied scientists agreed in ranking personal recommendations and cited references (books, papers) at the top of their lists of indirect sources of information, but they disagreed on relative order, applied scientists depending more on personal recommendations and pure scientists depending more on cited references. Indexes, abstracts,

bibliographies, and card catalogs -- basic elements of the formal information system -- were less often mentioned.

(8) Pure scientists, more than applied scientists, valued informal conversations with colleagues outside the university. Most regularly attended meetings of scientific and professional organizations to which they belonged. "Of this number the vast majority stated that the information gotten at meetings came from informal conversations rather than from hearing papers presented. . . . Scientists attending meetings generally found out who was doing work related to theirs, and what progress was being made. They were alerted to past and future papers and reports by colleagues in similar fields of interest. Little information of any significance was obtained by the pure researchers from verbal sources within their own organization [!]" (p. 234)

Thorne, 1954. The reading habits of scientists at the Royal Aircraft Establishment were studied by means of a diary and supplementary questionnaire. Only 30.3 per cent of the initial sample cooperated by filling in the diary daily for a week. This amount of self-selection in the final sample may be expected to bias estimates, but in an unknown direction and to an unknown extent.

University of Michigan, Survey Research Center, 1954. A questionnaire sent to 7104 physiologists (response rate: 75.9 per cent) was concerned partly with information flow. In addition to empirical use questions, respondents were asked to express opinions on the adequacy of various information tools. Respondents were most satisfied with journals, least satisfied with conferences. The problems they felt were hampering their ability to keep abreast of the field were ranked in this order: too many publications and too large a field; lack of access to published material; slowness of publication; inadequacy of publications; inadequacy of abstracts, indexes, and reviews; isolation from colleagues.

Shaw, 1956. Chemists, physicists, engineers, and botanical scientists at the Forest Products Laboratory of the U.S. Forest Service participated in a diary study of technical reading and library use. There appears to have been no response-rate problem: "To avoid any bias that might be introduced by asking volunteers, all professional research workers took part in the study." Respondents were promised anonymity to reduce the likelihood of a desirable-behavior bias.

Following Bernal's lead, Shaw collected personal data with a questionnaire and employed two types of diary cards. Again like Bernal, Shaw tabulated certain user attributes (rank, field) against reading behavior without taking account of interactions among these attributes. In addition to the obvious boon of an almost-perfect response rate, Shaw's study improved upon Bernal's in its inclusion of a broader range of reading behaviors and in its determination of the respondent's information-seeking purpose and of the source of the reference that led him to a given information-seeking act.

Shaw greatly enhanced the reliability of his findings by repeating the diary study with the same group of scientists one year later. The need for replication was realized midway through the first study, when a check of materials known to have been in the hands of the research staff showed that only 43 per cent had been mentioned in the diaries. Meetings with cooperating scientists at this point yielded fuller records during the second half of the period covered by the first study, but omissions were still found. Therefore (Shaw states), "since it appeared that the diary method, even with the best of intentions, could not be depended upon for completeness, the period was reduced to one month in the second check to see whether more complete reporting would result from a shorter period." (p. 34)

Comparison of tables for the two time periods shows great uniformity in reading behavior. Even the discrepancy between estimated time and actual time spent on library materials remained about 50 per cent, leading Shaw to conclude that "the diary method, even with better than average cooperation and superivision, is not reliable enough to justify further studies over extended periods of time." (p. 60)

Some of Shaw's findings (data from the second study):

(1) Rank of the scientist is related to at least two information behaviors. Higher ranked scientists

subscribed to more journals and spent more time with library materials.

(2) Primary print information sources in three fields (physicists excluded because of insufficient cases), ranked according to frequency of mention in the diaries were:

	Chemistry	Engineering	Bot. Science
Trade journal	2	1	1
Research journal	1	2	2
Book other than hand-book, dictionary, etc.	3	4	3
Abstract	4	5	4
Bulletin, U.S. non-government labs	5	3	7
Bulletin, non-U.S. labs	-	-	5
Report, Forest Service labs other than FPL	-	-	6
Bulletin, U.S. military agency	-	6.5	-
Bulletin, other U.S. government labs	6	6.5	8

Although archival sources head each list, from 11.4 per cent (chemistry) to 21.9 per cent (engineering) of all print sources entered in diaries were bulletins, reports, and other occasional publications.

(3) Publications sought by the researchers conformed to the "standard curve" of age that will be examined more fully in Part IV:

Age of publication in years	Percentage of diary entries
< 1 year	37.6%
1-2	9.1
2-3	5.7
3-5	11.9
5-10	10.1
10-20	8.8
> 20	5.9

(4) Within the category of "specific information" as a reason for consulting a print source, researchers listed "results", "method", and "theory" as the desired content, in that order.

(5) It was found in Urquhart's study (1948) that verbal recommendations accounted for only 15.9 per cent of all references that led to information-seeking, as against 32.8 per cent of all references found in abstracts and digests. Shaw found that personal recommendations accounted for 24.3 per cent of all references that led to information-seeking, while only 7.6 per cent came from abstracts and reviews (the category most nearly comparable to Urquhart's "abstracts and digests").

If it is not artifactual, a reversal of this magnitude demands further analysis: Are there British and American "styles of investigation" in which print and interpersonal sources are valued differently? Did the research environment at the Forest Products Laboratory encourage a pooling of knowledge of the literature? Although the scientific fields represented are about the same in both studies, were the types of research conducted at the FPL more poorly abstracted and reviewed? Data to answer these questions are not available.

Maizell, (1957) 1960. A sample of 94 research chemists in a single industrial laboratory was divided into three groups on the basis of "creativity", and the information-gathering behavior of the high (n = 26) and low (n = 32) groups was studied. These remarkably cooperative scientists answered a 70-item questionnaire on information use, kept a diary for ten days, completed two written tests of "creativity", and supplied Maizell with lists of publications and patents they were responsible for. In addition, ratings on a 20-point scale of each scientist's "creativity" were obtained from supervisors who worked with him.

The weakest element in this study was the measurement of creativity. Realizing that the validity of his creativity scores could be challenged, Maizell administered the two written tests, obtained the supervisors' ratings, and also collected the lists of publications and patents. Dividing the sample on the basis of averaged supervisors' ratings, he found that creativity-test scores of the high-rated and low-rated groups were significantly different at the .01 level. The high group also had a higher average number of publications and patents, but this difference was not tested for statistical significance.

Concern for the validity of a measure is rare in the literature of use studies, and Maizell made a conscientious attempt at construct validation. Yet an <u>index</u> of creativity based on all measures would have been superior to supervisors' ratings alone (in some instances, only one supervisor rated a given chemist). It is true that the high-rated and low-rated groups were significantly different on both creativity tests, but this demonstration is somewhat irrelevant. What is important is the <u>strength</u> of the agreement. For instance, a Pearson correlation of only .26 between supervisors' ratings and creativity test scores for all 94 scientists would be significant at the .01 level, but it is too low to validate either measure. An index of creativity could have been computed from all the measures jointly, the contribution of each measure to the index weighted by its average intercorrelation with other measures. By this procedure the measure that correlates strongly with other measures contributes heavily to the index.

Assuming that supervisors' ratings do reflect on creativity, however, the following differences between the high and low groups are of interest (Maizell's own summary, p. 13):

(1) The most striking differences . . . pertain principally to the reading of technical literature on the job. The most creative chemist does significantly more technical reading on the job than the least creative chemist [i.e., the two groups contrasted].

(2) Technical information services offered by the library staff were only of moderate importance to the most creative chemists. Some of the most creative chemists used these services, but most of them relied in large measure on their own efforts.

(3) There were no important differences with respect to nontechnical literature, or reading at home.

(4) The most creative chemists did not give clear evidence that they had a more critical approach to the literature than did the least creative chemists. The only suggestion of a more critical approach by the most creative people is their somewhat more frequent need to verify data found in the desk handbooks.

(5) The most creative chemist is not reluctant to use literature sources which are more difficult to consult, such as the older chemical literature, the more scholarly literature, and advanced treatises and monographs. Also, he is interested in technical fields other than his own immediate specialization.

Although it is not clear in the 1960 publication, Maizell's thesis (1957, unpublished) shows that education and creativity make independent contributions to these information-gathering behaviors.

Menzel, 1958. Summarized at length in Part III.

Fishenden, 1959. The interesting combination of diaries and personal interviews provided data on methods by which information was found by a small sample of researchers at the Atomic Energy Research Establishment, Harwell, England. Although there were only 63 diarists (50 of whom were later interviewed), the diaries were kept for approximately two months, and 1896 information-seeking acts were recorded.

Fishenden felt that previous lack of success with diaries (Bernal, 1948, and Shaw, 1956) could be attributed to unnecessary complexity in record-keeping format. He designed a single card on which the scientist could record successive information-seeking acts with simple tallies showing simultaneously how the information was found and whether the information was contained in a report, a published paper, a review, or a book.

Results were cross-tabulated by rank (junior staff vs. senior staff) and by primary research activity (pure vs. applied). Educational level of the sample was predetermined by including only those staff grades containing honours graduates.

In addition to the four formal print sources, the diary card provided space for recording information received via personal communications (written and spoken), lectures, and conference proceedings.

Diarists could not decide on a logical basis for dividing such information into entries, however, and the data are not tabulated.

Data from the personal interviews largely corroborated diary records of the formal print sources. Another check showed that the records were 80 to 90 per cent complete and that they "presented a valid picture of the information used."

Fishenden's Table 4 can be re-analyzed to point up a problem often overlooked in diary studies:

Cumulative percentage of all 1896 entries	Cumulative number of people contributing this percentage of entries
10	2
20	4
30	7
40	10
50	14
60	20
70	26
80	33
90	45
100	63

That is, just two people account for 10 per cent of all diary entries by themselves. Fourteen people, or 22 per cent of the sample, account for 50 per cent. Although the nominal sample size is 63, just a handful of active diarists provide most of the entries. Cross-tabulations relating information use to other variables will be greatly affected by the classification of this small number of researchers.

A distribution as skewed as this indicates a need for case studies. When two people control 10 per cent of the data, their possible idiosyncrasies should not be ignored.

Glass and Norwood, 1959. The title, "How Scientists Actually Learn of Work Important to Them", with implicit emphasis on the adverb, is intriguing, but this pilot study reports too little data to fulfill the title's promise. It is clear from the text, however, that the authors merely wished to call attention to the fact that the formal system of bibliographic tools provides little useful information according to scientists themselves.

Fifty scientists, scattered in 15 fields, were interviewed. Each scientist was asked to select a recent, significant paper from his own list of publications. From references given in the paper he was asked to choose up to six items "representing scientific concepts and research of major or crucial significance to the development of his own work reported in the chosen paper." He was then asked two questions: (1) How did you first learn of the existence of the work reported in each of the selected items? (2) Would it have made any significant difference to the progress of your own work had you learned of it sooner than you did?

The five "actual" sources most often mentioned were (in descending order): casual conversation, a regularly scanned journal, a subscribed-to journal, a cross citation in another paper, a reprint received from the author. These five sources accounted for 70 per cent of the items. The first source alone accounted for 24 per cent, the first two sources 48 per cent.

As Glass and Norwood acknowledge, "dependence upon the memory of the scientists interviewed constitutes a flaw in the present procedure." If a scientist has been pursuing a line of research for several years, it seems very unlikely that he could recall the sources from which he first learned of the "concepts and research of major or crucial significance" that the authors chose to focus upon.

Herner, 1959. The information-gathering behavior of 500 medical scientists, affiliated with 59 medical research institutions in 6 cities, was studied by means of personal interviews. The primary purpose of the study was to determine what use these scientists made of Soviet medical research information. However, enough additional information-gathering behaviors were investigated to make this a better-than-average general use study.

In response to the question, "How do you generally keep abreast of current scientific developments in your field?", only 8 sources were mentioned by 10 or more (i.e., 2 per cent or more) of the scientists. The three dominant sources -- regular scanning of research journals, attendance at meetings and lectures, and face-to-face contact with colleagues -- accounted for 77 per cent of all responses to this question. The remaining five of the eight were indexing and abstracting publications, textbooks, review papers, correspondence with colleagues, and visits to other research organizations.

Another set of questions established a recent "critical incident" in which each scientist needed information to solve a problem, then determined which sources the scientist turned to. Herner tabulated types of problems against sources-turned-to and thereby demonstrated that different problems lead to different information-seeking strategies. Over-all, 1.9 sources were mentioned per problem, suggesting that the first source consulted did not always yield a sufficient answer. Personal contacts, journals, and indexing-abstracting publications were the most mentioned sources for answers to problems, in that order.

In response to the question, "Do you recall where you got the idea (or inspiration) for your present or most recent project?", the scientists mentioned their own previous work first, then colleagues, "reading literature", and "observation of patients". (This last source of ideas is a familiar one to behavioral scientists, substituting "people" for "patients".)

The scientists were asked how they learn the existence of, or locate, publications or other sources of information which might be useful. These five methods were each mentioned more than 400 times: cited references (including footnotes), "chance or accident", indexing-abstracting publications, personal recommendations, and personal reference files.

The "critical incident" approach was used again to determine how each scientist went about conducting a literature search concerning some recent problem or question in his work. The five primary sources of assistance were journals, indexes-abstracts, cited references (including footnotes), colleagues, and texts-monographs.

Herner's interpretation of these data place his study in the "middle epoch" of information-flow research (cf. p. III-16 infra): "The primary conclusion that can be drawn from the foregoing paragraphs is a reaffirmation of the significant role of personal contacts in the getting and transmitting of scientific and technical information."

Hogg and Smith, 1959. Engineers, physicists, mathematicians, metallurgists, and biologists in the Research and Development Branch of the United Kingdom Atomic Energy Authority's Industrial Group establishments participated in a diary and interview study of information use. All 157 scientists were interviewed, and 144 of the diaries were returned.

Although the diaries were kept for only two weeks, a brief period in comparison with Fishenden's two months, the authors are candid enough to admit problems of reliability and validity in the records. The reliability problem: "The overall accuracy depended largely upon the diarists completing their records at the time of reading (which they were specially asked to do) but the neatness of many records gave rise to suspicions that they were marked up afterwards, or that scientists are tidier workers than is often supposed!" [italics theirs] The validity problem: ". . . it was evident from remarks made during some of the interviews, that many had postponed diary records until they were free to do some reading." If some scientists kept diaries faithfully during the assigned period whether they were reading much or little, while other scientists saved the diaries for a period in which they read much, the composite results are invalid. Perhaps the putting-off behavior explains why these scientists averaged 7.0 entries per person per week while Fishenden's sample (also employed by the U.K.A.E.A.) averaged only 3.8.

The interviews probably yielded more valid data than did the diaries; the investigators were favored with 100 per cent cooperation in this part of the data collection. The interview schedule got off to a bad start, however: "Assuming that you do some scientific or technical reading during working hours, are you often without adequate time for it?" Not surprisingly, 73 per cent of the sample answered, "Yes, I am often without adequate time for it." The next question in the schedule continued in this negative vein: "For about how many weeks in the past year were you unable to do any reading during working hours?" The average of the responses was 27 weeks, or 58 per cent of the working year. Unless working conditions in the U.K.A.E.A. Research and Development Branch are quite unlike those of other research environments studied, such an estimate of weeks without "any reading" during working hours is incredible. Of course no scientist can reconstruct an entire year's reading behavior, even if the question were phrased positively as standard practice in behavioral research dictates. Yet intuitively it seems that "almost every week" or even "every week" would be the modal response to the positive question, "For about how many weeks during the past year were you able to do some reading during working hours?"

Some findings of interest in the Hogg and Smith study include:

(1) The scientists were asked to evaluate the usefulness to them of eight formal and informal sources of information. At the top of the "very useful" list were "relevant reports and Committee technical papers" and "contacts with others in your field", followed closely by "relevant books" and "relevant journals". Attendance at "external conferences and professional meetings" was lowest in rated usefulness of the eight.

(2) References that led to information-gathering recorded in the diaries came primarily from other scientists. Personal references were three times more frequent than references from other journals and books, four times more frequent than references obtained from abstract journals and the library catalog (taken together).

(3) Articles in periodicals were the modal source of reading "for general interest". Reading "for your current or future research commitments" was concentrated in reports, followed by textbooks and periodicals.

Scott, 1959. Personal interviews were conducted with 1082 technologists in the British electrical and electronics industries. Without debating the point that these technologists were not all scientists (the sample covered "the whole range of technical activities from foreman level to research director"; "61 per cent had no academic or technical qualifications"), this study deserves to be reviewed because it was done well.

The study was designed and supervised by Leslie T. Wilkins; Scott was responsible for the analysis and report. Acknowledgement is made to Saul Herner, some of whose questions were borrowed for the Wilkins-Scott interview schedule. The entire methodology -- personal interviews, well-structured interview schedule, large sample, "critical incident" approach, attention to both formal and informal systems -- is reminiscent of Herner's own work. It is a compliment in this field, however, to imply a "steal" of sound precedent methodology. Other things being equal, comparable data are obtained when the same, not similar, questions are asked in successive studies.

Herner's and Scott's 1959 findings are more nearly comparable than findings obtained from other pairs of independent studies, and therefore it is instructive to note the limits of comparability:

(1) Herner sampled 500 medical scientists in 59 medical research institutions; Wilkins and Scott sampled 1082 technologists in 127 industrial firms. Both used random sampling procedures; both (apparently) obtained almost perfect cooperation from the sampled groups.

(2) Data collection procedures were essentially identical

(3) The two samples differed in nationality, in area of research, and especially in academic background (17 per cent of Scott's sample, versus 100 per cent of Herner's, held degrees).

(4) A large percentage of Herner's sample, but a very small percentage of Scott's sample, could be described as pure scientists.

Several of these differences could be held constant in the analysis. For instance, degree-holding pure scientists in each group could be compared. If only one uncontrollable attribute remained (e.g., nationality), its relation to information use could be examined by holding all other attributes constant. In these two studies, unfortunately, both nationality and area of research cannot be controlled, and the most powerful comparative analysis could only establish that nationality and/or area of research was related to information use, all other attributes held constant. Of course neither investigator can be faulted for limiting comparability. Practical factors determined which groups would be studied, and at least the methodologies were comparable.

Scott's demonstration of a cluster of 24 positively intercorrelated variables was a good first step toward multivariate analysis. He reports also that a factor analysis "carried out on part of the data" established what he calls a "general activity factor", but the analysis itself is not presented and we cannot determine whether variables of general significance were heavily loaded on the factor (nor is the initial correlation matrix presented). Some of the 24 items are of general significance (e.g., "attends meetings of technical or scientific societies"; while others are so special that they must certainly have been added to the analysis <u>post hoc</u> on the basis of intercorrelation alone (e.g., "tends to read journals which do not contain advertisements for jobs").

With his large sample it would be interesting to know the patterns of partial correlations among such variables in the matrix as these:

Academic or technical qualifications

Age

Readership of many or few journals

Recall of a useful article recently read

Use of literature as first step in solving current problem

Readership of journals outside primary field

Readership of "difficult" journals

Attendance at meetings of technical or scientific societies

Attendance at conferences or courses

Chance acquisition of useful information

More interesting than Scott's findings, which will not be summarized, are the inferences he draws from them. He reveals a strong disposition, by no means common in this field, to accept the information economy he finds as a viable one. He begins with the inference that "the main function of the technical literature is not that of a reference source for consultation but a primary source of stimulation." Again: "It is suggested that the principal role of the literature is to supply useful information _which is not being deliberately sought_ by the reader. Compared with this, its role as a reference source is a good deal less significant." [italics his] His evidence is derived from responses indicating that: (1) _few_ researchers in the sample turned to the literature to solve a "critical incident" problem, (2) most in the sample mentioned the literature as one of the most important sources of ideas, (3) only a quarter or so of the "recent useful articles" were deliberately consulted by the researcher "on his own initiative in order to find a definite piece of information"; the others were encountered by chance or mentioned by colleagues, (4) few in the sample could give the title of an abstracting journal used during the three-month period preceding the interviews, and an analysis of reasons for using the abstracts showed that "they were used very much more often for news than for searches." (5) more than half of those whose firm had a library did not use it; "this becomes explicable if they regard the technical literature not as a fund of information to be consulted but as a source of primary stimulation" (they were exposed to some journals, by subscription or via a circulation list, independently of the library -- Scott argues that this small set of journals would be sufficient for stimulation but insufficient for reference).

Scott states his nonreformer's bias in these terms: ". . . in the next decade or two we had better take the scientist broadly as we find him and build our system of information storage around him." After presenting his data, he continues, ". . . if it is true that the technologist, when reading, is seldom searching for anything, but is reading for whatever he might find, then it seems clear that any improvement in the organization of the literature for reference will be of relatively marginal value in increasing the amount of communicati...

Much more might be achieved by contriving that the important material be presented to the technologist in the place where he will see it in his routine reading and in the manner in which it will attract his interest."

Scott may even have drawn the wrong inference from his findings: perhaps scientists _would_ use the literature more for specific reference _if_ it were adequately organized. The interesting fact about this discussion is that he has made his peace with information-gathering behaviors that many documentalists consider to be irrational and in need of correction. Scott was an early exponent of the user-accomodating system described abstractly by Paisley and Parker (1965).

Törnudd, 1959. The strengths of Törnudd's report are her careful survey of earlier research and her concluding statement of seven factors that appear to be influential in scientific information-gathering (accessibility of information, kind of work, working environment, education background, field of science, nationality, and age), even though information flow has not been "proved to be influenced" by these factors as she asserts. Another strong point was the high response rate she obtained using mail questionnaires (cf. her similar success in the 1953 study).

The principal weakness of her study was the size of the sample relative to the number of attributes she was trying to keep track of. Her 188 respondents represented 2 countries (Denmark and Finland), 3 research settings (academic, industrial, and research institute), 25 research specialties, pure versus applied research, and 4 types of advanced degrees. Cross-tabulations yielded frequencies much too low for stable percentaging, although percentages were computed. The alternative procedure, summing over other attributes for a single-attribute breakdown, yielded ambiguous data in light of known disproportions of (for instance) persons with advanced degrees in industrial, academic, and research institute settings. Her findings are generally within the range established by such precedent studies as Bernal (1948), Herner (1954), and Shaw (1956), but discrepancies cannot be regarded as reliable.

Törnudd's study is inconclusive for a commendable reason. Because she assumed that information flow has many determinants, she attempted to study six factors simultaneously (age was held relatively constant). Ten times the sample she drew would have been marginally adequate for such a study.

Kotani, 1962. In a questionnaire study of information use involving 278 Japanese scientists, Kotani found much the same joint dependence on print and interpersonal sources that has been observed in use studies in the United States and Europe. Marked differences were found among different fields of Japanese science, but interpretation of these findings is impeded by the usual haphazard distribution of other attributes within fields.

The Japanese scientists, like Törnudd's Danish and Finnish scientists, felt they were handicapped by the cost and labor of obtaining information written in the "world languages" and of publishing their own work in languages other than Japanese.

Mote, 1962. Studies based on examinations of information requests were defined beyond the compass of this review in Part I, but Mote's study of information requests received by the Technical Information Division of Shell Limited's Thornton Research Centre points to a generalization of potential significance in behavioral science information flow.

Mote classified the scientists making use of the services of the Technical Information Division into three groups:

(1) "The first group comprised occupations in a subject of which the underlying principles are well developed, the literature is well organized, and the width of the subject area is well defined . . . . A typical example of such an activity would be the search for the structure or the synthesis of a complex organic polymer [i.e., the scientists engaged in the activity are all organic chemists, and they are concentrating on just one aspect of organic chemistry]."

(2) "In the second group the subject area is wider and the information less well organized. The same hypothetical chemist as before could now be thought of as joining a firm engaged in research into the application of lubricants where the 'pure' science aspect of the work previously described is, to some extent, left behind; the work is now concerned with both chemistry and physics in an engineering environment. . . . The

literature is now less clearly organized for his purposes than before; relevant information will be found, to a greater extent, in the unpublished reports of industrial firms and government departments, in the proceedings of many more professional societies, workshop manuals, specifications, etc., in addition to that contained in the published literature."

(3) "The third group is really an exaggerated form of the second, in which the number of different subjects is greater, the type of problem to be faced by the scientist being subject to greater variation, and the organization of the literature being almost non-existent. This is not to say that the literature itself does not exist, but the degree of organization for the intended purpose, is, to say the least, unhelpful. [An example would be] an inquiry into the thermal properties of frozen soils." (pp. 170-171)

Of the staff at the Thornton Research Centre, Mote could identify only seven as clearly eligible for Group III. He then sampled seven scientists randomly from each of the other two groups.

The Technical Information Division's inquiry records were searched through a period of 18 months to determine the number of information requests received from each of the 21 scientists. Scientists in Group I proved to have submitted 3 requests, scientists in Group II 28 requests, and scientists in Group III 44 requests. A secondary search of "short inquiry" records for three months showed that the number of "short inquiries" received from the three groups was 4, 7, and 17, respectively.

To obtain more reliable data than his sample of 21 could provide, Mote classified all 178 graduate scientists at the Centre into the three groups and computed the average number of information requests received from each group. The median number of inquiries received per person in Group I was 1; in Group II, 3; in Group III, 15. Ranges of the information-request distributions for Groups I and III did not even overlap: no scientist in Group I made more than six requests, and no scientist in Group III made fewer than ten requests.

Mote concludes from these findings: ". . . perhaps not only individuals but also organizations, of a technical or research type, might be subject to the same groupings, and that this might explain, in part, the differences between different technical libraries and information services."

The implications of this study for information flow in the behavioral sciences and in their interdisciplinary offspring are intriguing. Does the social psychologist in fact process more information than his colleagues whose interests are more central to psychology or sociology as traditionally defined (e.g., the experimental psychologist, the rural sociologist)? Even if Mote's findings do not imply that Group III scientists processed more information over-all than Group I scientists (the only datum is that they submitted more information requests), it seems intuitively reasonable that the scientist whose interests cut across two or more traditional subject areas may have to process more information over-all than the single-discipline scientist.

Garvey and Griffith (American Psychological Association), 1963-. Summarized at length in Part III.

Appel and Gurr, 1964. Wisely describing their study as a "pilot survey" of the "Bibliographic Needs of Social and Behavioral Scientists", Appel and Gurr report data from 66 anthropologists, economists, and psychologists (representing only 28 per cent response in their sample) pointing, in their interpretation, to the conclusion, " . . . for the social sciences as a whole, effective bibliographic retrieval systems are an imperative. It is none too soon to explore the multitude of elements involved in planning such systems, for the future scope and nature of the social sciences will be vitally affected by them."

Without debating the point that we all look forward to comprehensive, up-to-date, easily searched, machine-stored bibliographies, abstracts, and even (some day) complete papers, it is surprising to find in these responses such demand for improvement of the formal information system that few social scientists extensively use (e.g., in this sample, only 30 per cent regularly used abstract journals such as Psychological Abstracts). An unresolved question raised by the recurring finding that formal bibliographic systems are little used is whether an improved formal system would justify its greater expense by converting nonusers into users. If the considerable research evidence about interpersonal information flow has been correctly interpreted by such investigators as Menzel (1958), Scott (1959), and Price (1963), the formal bibliographic systems fall into disuse because interpersonal systems in the age of "invisible colleges and the affluent scientific commuter" (Price's coinage) are more responsive to particular information needs of individual scientists. Price asserts: " . . . one of the great consequences of the transition from Little Science to Big Science has been that after three centuries the role of the scientific paper has drastically changed. In many ways the modern ease of transportation and the affluence of the elite scientist have replaced what used to be effected by the publication of papers. We tend now to communicate person to person instead of paper to paper. In the most active areas we diffuse knowledge through collaboration." (1963, p. 91)

Perhaps because their questionnaire focused narrowly on print information sources, Appel and Gurr obtained print-oriented responses where previous studies showed a balanced dependence on print and interpersonal sources (in many studies the balance tipping toward the latter). Whether or not their findings are valid, they and the journal with which they are affiliated have undertaken an ambitious follow-up that may settle the issue. In launching its Universal Reference System ("a computerized documentation and information retrieval system"), <u>The American Behavioral Scientist</u> is in effect testing the proposition that scientists will make substantially greater use of an improved formal bibliographic system.

Berul, Elling, Karson, Shafritz, and Sieber (The Auerbach Corporation), 1965. This study of the information-gathering behavior of Department of Defense research, development, test, and evaluation personnel has not yet received the attention it deserves as a major contribution to information-flow research. Even if its substantive findings cannot be generalized to the research settings in which behavioral scientists usually find themselves (the scientists in this sample worked in military installations, with special information facilities available to them and special constraints imposed on the dissemination of information), the soundness of its methodology and analysis marks this study as unique.

The Auerbach Corporation was awarded a DOD contract "to determine how [RDTE personnel] acquire and utilize technical and scientific information in the conduct of specific tasks associated with the work." The Auerbach investigators began by compiling a 700-item bibliography of studies more or less related to information flow, hoping perhaps to adopt the methodology of precedent studies. They concluded that use studies in the bibliography "were limited by a narrowly or improperly drawn sample, by a faulty methodology, or by the number of questions asked about the use made of the information." But with faint praise they concede, " . . . these studies did aid in the development of the study methodology insofar as they served as a base to demonstrate where previous work had failed or had gained only limited success."

In fact their methodology has roots in the well-structured personal interviews that Herner introduced to this field (cf. Herner, 1954; Herner, 1959; Scott, 1959). The Auerbach study significantly improves upon these precedents only in the sensitivity of its multivariate analysis of the interview data.

From a population of about 36,000 scientists and engineers the Auerbach group drew a simple random sample of 1375, located in military installations in several parts of the country. Eleven Auerbach and National Security Agency interviewers were thoroughly briefed on interviewing procedures and on previous information-flow research (four of them even compiled part of the bibliography). Pretests of the interview schedule also provided the interviewers with field practice. In the final data collection these interviewers traveled to each installation in which there were sample personnel and obtained several interviews in a single visit. Subsequent visits ensured that the over-all completion rate would be quite high (88 per cent).

The decision to draw a simple random sample was made by default, insufficient information being available to stratify the population on attributes of interest. It would have been better, given the information, to stratify at least on educational background and on field of research activity, since only 8 per cent of the sample held the Ph.D. and engineers were overrepresented (in terms of number needed for reliable population estimates) while biologists, physicists, chemists, etc., were in short supply.

A "critical incident" approach was used to focus attention on a specific series of information-gathering behaviors. The concept of a "chunk of information" was introduced and defined as "the smallest quantity of information required to answer a task-related question." The investigators acknowledge that this definition has only heuristic value, but they wanted a small unit of information quantity which the respondent could use to partition a complex information-gathering problem into discussable subproblems.

The function of a chunk, the field of research it was drawn from, the source from which it was obtained, time required to obtain it, depth of information it conveyed, and its value to the task were principal dependent variables tabulated against such antecedent variables as educational background, field of research, kind of task, task output, etc. In a well-planned but somewhat dense presentation of data (partly photographed computer output with rows and columns identified only by code numbers), the simple frequency-percentage distributions of each behavior and attribute are followed by a judicious selection of two-way and three-way tabulations. As an example of a three-way tabulation, first source consulted is arrayed against function of chunk and field of chunk. Higher-order analysis would have been desirable (e.g., educational background as an additional control), but even the three-way tabulations begin to use up frequencies from which stable percentages can be computed.

A unique feature of this analysis is the authors' use of a "question value code", from 1 to 4, to distinguish reliable and valid questions from not-so-reliable and not-so-valid questions, according to post hoc judgment of a question's ambiguity, objectivity, codability, etc. The question value code is used as a flag on each cross-tabulation to remind the reader of the investigators' judgment of the over-all reliability and validity of the table. Such a code is not at all the same as a test of statistical significance, which cannot reflect on the reliability and validity of a table; it is an interesting innovation that ought to be continued and refined (i.e., the reliability and validity of the question value code itself should be assessed).

An attempt is made in each tabulation to express the meaning or implication of the frequency distribution verbally. A few of these comments can be excerpted to present some of the study's findings and to illustrate the cautious, non-inferential level of discussion:

Single-attribute tabulations:

(1) "In 52 per cent of the searches for information, the person first used a local source, such as a colleague, his own files, or local department files. More than half the 21 per cent blank answers are accounted for by information that came from a person's previous knowledge. Libraries and information centers were seldom used as a first source of information [only 5 per cent of 4687 chunks of information]."

(2) "Question 43 was an attempt to find out why the first source indentified [above] was used. Responses to this question are considered good because it is an easy question for the respondents to understand. The data in this table, however, are considered quite marginal because it was found that the categories were not mutually exclusive."

(3) "Question 47 was designed to establish how chunks were actually used in the task. It was expected that different types of chunks might have different use characteristics. The data, however, did not support this hypothesis."

Two-way tabulations:

(4) [Highest degree and field vs. source] "This table shows that people with bachelor's degrees in engineering tend not to use journals to obtain information, whereas people with advanced degrees in science tend to use both journals and texts." [No explanation is given for the confounding of degree and field.]

(5) [Highest degree and field vs. first source] "This table shows that there is no significant relationship between a person's highest degree and field and the choice of the first source to obtain information."

(6) [Field of research vs. first source] "This table shows that there are no unusual relationships between a person's occupational class and the choice of a first source to obtain information."

(7) [Kind of activity vs. first source] "As shown by this comparison, libraries were seldom used as a first source of information; however, the statistics show that they were more often used by research people and less by research and development support personnel."

(8) [Man-days of task vs. first source] "The data in this table show the time to perform a task has little or no effect on the choice of the first sources of information." [i.e., researchers were as likely to consult colleagues to begin a long-duration task as to begin a short-duration task]

(9) [Source vs. desired depth of information] "This table shows that there is no preference for the use of one [source] over another as a function of the depth of the information desired."

Three-way tabulations:

(10) [Field of task vs. field of chunk vs. first source] "This series of seven tables shows the relationship between the field of the task, the field of the information chunks, and the first [source] contacted to acquire the information. The analysis of these

tables indicates that there are no significant relationships between the field of the task, the field of the chunks, and the use of any particular first source to acquire the information."

As several of these comments suggest, a finding of no-significant-relationship was very common. The authors argue now and then in favor of the null hypothesis (i.e., that, since no significant difference was found, the sample was in fact homogeneous with respect to that behavior). For example: [nature of task vs. first source] "This table shows relatively few outstanding features, which is, nevertheless, significant since it implies that there is little or no relationship between the output or nature of the tasks and the use of the first source to obtain information." With this kind of interpretation a table is always significant.

Minor objections aside, the DOD study has greatly advanced this field of research. The method of data-collection and the strategy of analysis chosen by the Auerbach group cannot easily be challenged. Gains can be made, however, in stratified sampling of more diverse populations of scientists, classified at least by educational background, by field of research, and by institutional affiliation.

Flowers, 1965. Mail questionnaires were returned by 3021 of 6194 physicists and chemists, for a response rate of 49.5 per cent. These scientists rated contact with other scientists as less useful for information than abstracts and original published papers.

The trend was even more pronounced among the Ph.D. group. A serious defect in a study so recent is the failure to specify "information for what?", since original published papers, abstracts, and other scientists provide different kinds of information fulfilling different functions.

Since, however, this is not the only British study that indicates greater dependence on print sources among scientists in that country, it is time for someone to investigate what may be a reliable difference in primary source preferences among American and British scientists.

McLaughlin, Rosenbloom, and Wolek, 1965. Engineers and other scientists in an American electrical corporation were surveyed by means of self-administered questionnaires. The initital sample size is not given, but apparently the 430 respondents represent a high response rate. Thirty post-questionnaire interviews were conducted to check on respondents' interpretations of the questions (no difficulties detected).

The investigators chose to study a single corporation with five research divisions because it "seemed a tractable microcosm in which we might gain understanding of the processes and problems involved in the transfer of technical information across organizational lines." The implication of this last phrase for the design of the study was that communication within the respondent's work section was explicitly excluded in the line of questioning. This is an unfortunate narrowing of the range of information-gathering behaviors to be measured. How can sense be made of a scientist's information-seeking "outside the room" (so to speak) without reference to what he is routinely able to learn "inside the room"?

Within its limitations, the study reports findings consistent with past research:

(1) Using a "critical incident" approach and inquiring about "most recent useful information" and "most useful recent information" (within the previous six months), the investigators found that interpersonal sources accounted for 55 per cent of most recent useful information and 59 per cent of most useful recent information, even when the respondent's fellow section workers are excluded as possible information sources.

(2) Altogether, interpersonal communication played a part in 72 per cent of all reported instances and in 84 per cent of those instances in which the respondent's own knowledge had not already directed him to a literature source.

(3) Chance acquisition of useful information occurred in one-third of all reported instances.

(4) "Cosmopolitan" and "local" patterns of information-seeking were distinguished. The cosmopolitan pattern (extensive use of written sources external to the company) was associated with: (a) experience with

the source discipline from which the information was drawn, (b) active readership of journals and/or authorship in journals, (c) employment in basic rather than applied aspects of research and development, (d) higher job rank, (e) lower company seniority. The local pattern (extensive use of oral sources within the company) was associated with the converse of these attributes.

(5) The five research divisions reveal potentially interesting differences in information-use patterns. They also differ in research activities and in the educational backgrounds of their staffs (from 36.5 per cent doctoral degree holders in one division to none in three divisions). In no tabulations are these confounding factors controlled.

(6) The breakdown into divisions is useful, however, in that it provides five independent replications for checking consistency in tabulations involving variables other than the division classification itself. There is a consistent trend across divisions, for instance, that oral sources are most used for information about fabrication, next most used for information about design, and least used for information about theory and experimentation.

(7) It was found that respondents were most likely to turn to written sources when they had previous knowledge both of the field from which the information was drawn and of the intended application of the information. They were most likely to turn to oral sources when they had previous knowledge of intended application but not of the field. (This suggests the information problem in which the scientist knows just what he wants but doesn't know where to find it.)

(8) There was a small but consistent trend in the direction of more use of interpersonal sources among those who regularly read _many_ rather than few journals. These high users of journals were also more likely to consult interpersonal sources outside, rather than inside, the company. They were somewhat more likely to consult university sources than were low users of journals.

(9) Those who had themselves published a large number of technical papers used interpersonal sources _more_ than did those who had published few or no papers.

One of the strengths of this study is the judicious use of higher-order assertions to tie together lower-order findings. For instance: "As technology becomes more applied there is an increased reliance on oral channels, less use of published documents and a corresponding increase in the relative reliance on unpublished written sources, a greater incidence of the acquisition of information through specific, oriented search, and less use of sources external to the company. These distinctions in work functions may explain the apparent differences between our study and the patterns reported in other user studies." As each of these assertions is tested against data generated in the study itself, an attempt is made to reconcile discrepant findings from earlier studies.

Wuest, 1965. This project, only one-fourth reported at the present time, will eventually compare four methods of measuring information-use behavior. A questionnaire study has already been completed. The "diary-work-sample method" will be employed next. "Subjective scaling techniques" will then be used to assess "the subjective utility of various information channels for various tasks." Finally, "in a controlled experimental situation, subjects will be confronted with a task which will demand use of a controlled body of information. Use patterns will be described directly from records of the subjects' activities during the experiment." (Current Research and Development in Scientific Documentation, No. 13, 1964, pp. 26-27).

To date, 110 faculty members and graduate students in the Departments of Chemistry and Metallurgy at Lehigh University have cooperated in the project by completing a long questionnaire concerning biographical data and an unusually comprehensive set of information-gathering behaviors. The same population (not a sample; the entire departments were recruited) will participate in subsequent studies in this project.

The frequency distribution of each information-gathering behavior is presented in the first report, together with cross-tabulations by field and by amount of experience in the field. Neither attribute yields particularly instructive tabulations, in the first case because summing over graduate students and faculty members does not seem justified (and there are different proportions in each educational level in the two departments), in the second case because amount of experience is a misnomer: the more experienced group was stipulated to include all those of professorial rank or higher; whatever experience in the field an instructor (for instance) actually had does not seem to have been considered in his assignment to the less experienced class.

The author's comments on the limits of the questionnaire technique are useful; these limitations will be more clearly marked when comparative data are obtained in subsequent studies. In a way, the questionnaire technique received a rather mild test in this first study; by inquiring only about habitual behaviors, which are easily reported, the study did not test the validity of the questionnaire technique for gathering "critical incident" data, in which the line of questioning sometimes makes unreasonable demands upon respondents' recall.

Studies of Information Flow in the Context of the Research Environment

Certain studies were not mentioned in the foregoing chronology because they form a subset of great interest. Some of these studies were regarded by their authors as use studies; others were conceived as studies of the scientist in his research environment without special reference to information flow. All of them discuss either information-gathering or information-dissemination but not always both. The studies reviewed in this section differ from most use studies in the richness of their data on the research environment and on the scientist's daily routine. Some previously mentioned studies included some research environment factors, however, and no rigid line can be drawn to distinguish use studies from research environment studies.

Hertz and Rubenstein, 1953. Several studies and several methods of study provided data on scientific communication in relation to such research environment factors as the composition of research teams. The project began with an exploratory (mail questionnaire) survey of research and development laboratories. Questions concerned the number of research personnel in each laboratory and the disciplines they represented.

Field studies of a subsample of the laboratories used three methods: (1) diaries, kept at intervals over a five-week period, in which a stratified sample of researchers recorded their activities during 15-minute time samples, with specific emphasis on communicatory activity, (2) questionnaires, dealing with habitual information-gathering behaviors, (3) personal interviews, focusing primarily on evaluations of information sources.

The questionnaire study indicated that persons of higher institutional rank are most-preferred sources for task-related information, handbooks and other desk reference materials are second, and persons of equal rank with the researcher are third.

The diary study yielded unique data on the distribution of communicatory activities throughout the work week. The average number of communications was two per hour, a low level that suggests the usual failure of diarists to keep their records complete. Two cycles of activity are especially interesting: communication is heaviest during the middle of the work-day and during the middle of the work-week.

Amount of communicatory activity varied with function in the group (e.g., higher among those with administrative functions, lower among those with design functions), with institutional rank (highest among supervisors, lowest among assistants), with the duration of the research project (slightly higher in long-term projects than in short-term projects), and with the size of the research team (highest in medium-size teams, somewhat lower in large teams, much lower in small teams).

Hertz and Rubenstein classify information by content and by source but not by function. Ways in which the research team obtains various types of information (classified by content) are discussed.

The project's methodology is defended by the authors in a realistic appraisal of the limits of each technique. Some tests of reliability (in terms of intermethod agreement) were provided for in the data collection.

Meltzer, 1956. This study is an excellent secondary analysis of a nationwide survey of physiologists reported elsewhere by Gerard (1958). Meltzer's dependent variable was productivity -- amount of information-dissemination via formal publications. He sought to account for differences in productivity on the basis of several research environment factors, including funds available for research, freedom in the choice of research problems, type of research organization (if the researcher had an organizational affiliation other than an academic department), and importance thought to be attached to publications in promotion. Age and salary were included in the analysis as control variables; education was not controlled, perhaps because the sample of physiologists was homogeneous on that attribute (no data on education are reported).

Productivity was operationally defined as number of research publications authored or co-authored by a respondent in the three years previous to the survey. Meltzer admits this criterion is crude in that it gives equal weight to books and to papers, to single authorship and to co-authorship. Validity of the criterion was checked by correlating number of publications with number of citations of the respondent's work in the Annual Review of Physiology. The correlation was reasonably high (.51), and a parallel analysis with number of citations as the dependent variable yielded "essentially the same results".

Meltzer first shows that affiliation with a research organization is not related to productivity by itself. Two other factors, funds and freedom, are positively correlated with productivity but negatively correlated with each other. In combination the two factors interact: "If the scientist reports that he has a very low amount of freedom, successive increments of funds appear to have relatively little effect on his productivity. If he has a large measure of freedom, then successive increments of funds are reflected in substantial increments of output. A similar situation occurs with funds: where the scientist has a very small amount of funds available for his research, successive increments of freedom do not materially affect the level of productivity of the scientist; but where the scientist has an ample supply of funds, the amount of freedom makes a big difference in his productivity level." When affiliation with a research organization is introduced as a control variable, the interactional trend persists.

Productivity in academic, industrial, and governmental research environments is shown to be related to importance attached to publications in promotion. About 85 per cent of the academic and governmental scientists said that papers count in promotion at least to some extent; about 55 per cent of these scientists were highly productive (5 or more papers in the three-year period). Only 40 per cent of the industrial scientists felt that papers counted in promotion, and only 30 per cent of them were highly productive.

After additional exploration of these factors, Meltzer arrives at this conclusion: "If the conditions under which the scientist works do not allow him intrinsic job satisfaction [partly equated with freedom], then providing him with the finest equipment and facilities [partly equated with funds] may not stimulate him to produce. On the other hand, even the most motivated of scientists are not likely to accomplish much if they are hampered by a severe lack of facilities to work with. Although we must be very cautious in social science when making metric comparisons, it is tempting to conclude from our data that the state of science will be better off when scientists have a 'medium' amount of each of these 'commodities' than it would be if scientists had a great deal of one class but very little of the other class [i.e., funds and freedom]."

Pelz, 1956. In a study of a government medical research organization, Pelz looked for factors in the research environment that might account for "performance" differences. "Performance" was evaluated by each scientist's colleagues; the composite judgment appears to imply productivity, quality, and creativity. Environment factors of interest to this review were amount of contact with colleagues and similarity of the scientist's own values and the values of his colleagues.

Each scientist stated the importance to him of nine factors associated with his job. Correlational analysis of responses indicated that three factors clustered to form a "science orientation" (stress on using present abilities or knowledge, freedom to carry out original ideas, and opportunity to contribute to basic scientific knowledge) while three other factors suggested a "prestige orientation" (stress on having an important job, association with high-level persons having important responsibilities, and sense of belonging to an organization with prestige in the lay community). The two value orientations were found to be uncorrelated.

Some of the scientists were in daily contact with colleagues; others were relatively isolated and interacted with their colleagues less often. Scientists who interacted daily with dissimilar colleagues were higher in performance rating than scientists who interacted daily with similar colleagues, but higher performance seemed to be associated with similarity in value orientation among scientists who were more isolated. Pelz infers, "These findings suggest that scientists benefit by frequent opportunities to exchange ideas with persons having different values."

Another index of similarity was based upon the previous work experience of the scientist and his colleagues. These medical scientists had worked variously in government, academic, and hospital settings. Those who had equivalent work experience were coded as similar. Again the highest performance rating was associated with lowest similarity given daily interaction; among scientists who interacted less, similarity was positively correlated with performance.

When the scientist nominated a "most important colleague" from the research team, however, the relationship between performance and similarity was reversed. Daily contact with a similar "most important colleague" was associated with higher performance than daily contact with a dissimilar "most important colleague", and the converse was true among scientists who interacted less often with their "most important colleague". This relationship held among junior-level and senior-level scientists tabulated separately.

Pelz himself suggests that the inferred dir tion of effect may be backwards. It may be that a high-performance scientist associates primarily with one like-minded colleague and then seeks variety in his other associations. Or the similarity-performance relationship may be artifactual: the same factors that lead to low performance may limit the variety of colleagues available for interaction. Barring that possibility, Pelz's study proposes for further consideration an interpersonal analogue of Scott's "literature for stimulation" hypothesis (1959) and Maizell's finding that more-creative scientists range farther afield in their reading (1960).

Ackoff and Halbert, 1958 (Halbert and Ackoff, 1959). The Case Institute of Technology, locus of this and the following study, has been primarily responsible for the introduction to this field of participant-observer methodology and the "ratio-delay" procedure for obtaining random time-samples of scientists' behavior.

In the Ackoff and Halbert study, approximately 25,000 observations were made of the daily activities of about 1500 chemists in 45 industrial organizations and 5 universities. The 1959 report, here reviewed, is actually the less complete, since it was prepared for the 1958 International Conference on Scientific Information.

It covers data from 18,000 observations of industrial chemists only.

Each chemist was observed at two random moments of time each day for nine consecutive days by a member of his organization trained in observation by the Case investigators. In each of the 18 observations the behavior of each chemist in the sample was coded into one of the following categories: (1) scientific communication, (2) non-scientific business communication, (3) thinking or planning alone, (4) equipment set-up and maintenance, (5) equipment use, (6) data treatment, (7) personal and social, (8) none of these, and (9) out of area. The last category was necessary because observers were instructed not to attempt to follow the chemist out of his laboratory area.

When the chemist was observed to be engaged in scientific communication, the activity was categorized according to channel, source person (or receiver), and phase of the communication activity (reading, writing, hearing, telling). Some of this information had to be obtained from the chemist directly.

Additional data on the research environment was obtained via a questionnaire completed for each laboratory by the observer (who worked there himself). Questions concerned type of research conducted, information facilities available, funding of the research, and the research specialties represented by the scientist's colleagues.

Some of the Ackoff-Halbert findings:

(1) The range of time spent on scientific communication by all chemists was 15.7 per cent to 61.4 per cent. Scientific communication used more time, on the average, than any other activity. It was the only activity with no zero frequencies (i.e., at least one person in each group was observed at one point to be engaged in scientific communication).

(2) The mean percentage of total time spent in scientific communication, 33.4 per cent, may be compared with means of 10.4 per cent in business communication, 6.0 per cent in thinking and planning alone, 6.2 per cent in equipment set-up, 23.4 per cent in equipment use, 6.4 per cent in data treatment, 9.8 per cent in personal and social activities, and 4.4 per cent in miscellaneous activities.

(3) The 33.4 per cent of total time given to scientific communication was divided into 19.4 per cent oral communication and 14.4 per cent written communication [unreconciled discrepancies between values presented in table, figure, and sum].

(4) Unpublished written materials received almost twice as much time as published written materials (9.5 per cent vs. 4.9 per cent).

(5) About two-thirds of the scientific communication involved other chemists. About one-fifth involved technicians, secretaries, and other nonscientific company personnel, and about one-ninth involved scientists other than chemists.

(6) The investigators performed the somewhat complicated analysis necessary to determine which activities were correlated and which were independent (the analysis is complicated because mutually exclusive and collectively exhaustive events are artifactually negatively correlated -- if one occurs, others do not). When the artifactual relationship is controlled, scientific communication is significantly correlated with business communication (+), thinking and planning alone (+), equipment use (-), and data treatment (+). The correlation between scientific communication and thinking and planning alone is the most significant and the most reciprocal, suggesting that both activities may be characteristic of the same phase of a research project (just as scientific communication and equipment use seem to be characteristic of different phases of a project, judging from the negative correlation).

(7) Time allocation to scientific communication was found <u>not</u> to be significantly related to the size of the research team.

(8) A test of the independence of the 18 observations made of each group showed a substantial grouping effect -- that is, what one chemist was doing was correlated with what other chemists in his group were doing.

Note that the unit of analysis in this study was the research group and not the individual scientist. No data were collected on personal attributes that would help to explain differences in information-use patterns. While most use studies fail to consider the research environment, this research-environment study fails to consider important attributes of individual users. In any event, the study would have to be done quite differently, with many more than 18 observations per person, to provide reliable data on the individual scientist.

Martin, 1962. In another Case Institute of Technology study, 297 chemists and 404 physicists served as their own observers with the help of "random alarm mechanisms" that alerted them to record their reading behavior about 3.5 times a day for 14 days. The "random alarm mechanism" (RAM) is quite ingenious; it is small, inexpensive, easily reset. It can be carried by the scientist anywhere, and therefore can provide a more comprehensive sampling of the scientist's behavior than a participant-observer is capable of. This latter advantage is important in an era of greatly expanded personal contact among scientists through travel. If an unusually cooperative scientist were to carry a RAM with him for a year, an unparalleled log of some 1250 observations could be obtained. A small sample of such case studies would contribute uniquely to the understanding of scientific information flow.

Since Martin's study dealt only with journal reading behavior, its findings are much less interesting than the methodological precedent it sets. Even so, because of the large samples of observations, these findings are striking:

(1) Based on 15,408 observations of chemists and 17,894 observations of physicists, it was found that the groups agree in spending just 2.2 per cent of their waking hours in reading scientific periodicals.

(2) In both groups, only ten journals account for half of all reading observations.

(3) Whereas 58.7 per cent of the physicists' observations indicated reading for specific information, the corresponding percentage for chemists was only 35.5, a highly significant difference.

Because of the ingenuity of Martin's procedure, it is sad to think of the 32,567 nonreading observations that went uncatalogued. Even superficial information on what scientists were doing at these sampled times would have permitted a partial replication and a partial extension of the Ackoff-Halbert study (replication of in-laboratory behaviors, extension to out-of-laboratory behaviors).

Allen, 1964. This is the most substantial report to date of a series of studies undertaken by Donald Marquis, Thomas Allen, and associates at the M.I.T. Sloan School of Management. The past year's research, incorporating several improvements in design, should be reported shortly.

Research in information flow often leads to inconclusive results because significant variables have neither been measured nor controlled. One of the most challenging design problems in such research is the control and/or measurement of these variables. The Marquis-Allen project is unique in its control of the task for which information is being gathered. Attempts have been made in previous projects to limit task variation by distinguishing between "pure" and "applied" research, between disciplines, and between types of institutions in which scientists work. Even with these constraints, however, scientists' tasks are extremely varied; the investigator can only hope that the information needs of the diverse tasks will balance out within compared subgroups of his sample.

It is in the nature of scientific inquiry that scientists do not duplicate each other's work. When occasionally a project is replicated, the information requirements of the replication are entirely different from the information requirements of the original project. Ingeniously, Allen and Marquis struck upon a research situation in which many teams of scientists and technologists are competing to find the best solution to a common task: preparation of research and development proposals in competition for a government contract.

Twenty-two proposal competitions for the Air Force and for NASA were studied. Questionnaires were sent to the managers of the 198 proposal teams involved. Satisfactory returns were obtained from

156, for a response rate of 78.8 per cent. The median number of replies for each competition was seven.

Correlations were computed among 12 variables concerning information sources, characteristics of the proposal effort, characteristics of the proposal team, and characteristics of the parent laboratory. An average correlation for each pair of variables was computed (via the $\underline{r}$ to $\underline{z}$ transformation) from the 22 intracompetition coefficients.

Among the findings reported by Allen:

(1) Information-gathering occupied 22 per cent of the total time given to the task by the 156 proposal teams. Total time spent gathering information proved to be <u>unrelated</u> to proposal <u>quality</u> as evaluated by the government agencies.

(2) Time spent in literature search was also found to be unrelated to proposal quality.

(3) Time spent consulting with in-laboratory specialists was also unrelated to proposal quality.

(4) Time spent consulting with <u>outside</u> specialists was <u>negatively</u> related to proposal quality.

(5) The intercorrelations among the three information sources and proposal quality is such that, although total time spent gathering information is uncorrelated with quality, the multiple correlation of quality and the three information components taken separately is

.71 (i.e., 50 per cent of the variation in quality is accounted for on the basis of information inputs). Since the square of the multiple correlation greatly exceeds the sum of the squares of the three zero-order correlations of information and quality, a pattern of interaction is suggested that deserves further study.

Partial correlations were also computed between information use and quality, holding constant such variables as size of technical staff, level of effort, time spent in analytic design, ratio of technical staff to total employment, etc. The partial relationships are not especially interesting, but these analyses more than justify themselves by testing several alternative explanations to the implicitly causal relationship between information use and quality.

Shilling, Bernard, and Tyson, 1964. This study of informal communication among bioscientists combines features of a use study and a research-environment study. Information about the research environment of 64 government, industrial, university, and private laboratories was obtained by means of interviews with laboratory administrative staff and printed reports dealing with the history, policies, and administration of the laboratories. Questionnaires completed by 673 scientists working in these laboratories provided data on personal attributes, on communication behaviors, and on their view of laboratory policies affecting information flow.

The strategy of analysis is admirable. First, informal communication practices were analyzed with respect to the individual attributes age and sex(as is always the case in these studies, the reader may wish that other attributes, such as rank within the laboratory, research specialty, and education, had been analyzed separately). Then, when the limits of explanation via these individual attributes had been drawn, the same communication practices were analyzed with respect to laboratory policies and other research environment factors.

This is the only study known to the reviewer in which communication practices (information inputs) are tested for association with laboratory productivity (information outputs). In parallel analyses laboratory policies are tested for association with productivity. Unfortunately in these tests the data are always aggregated by type of laboratory (government, industrial, etc.), a gross unit of analysis in which significant relationships may be concealed.

Although most of the analysis is carefully handled, the reader should beware of the following assertion, which might otherwise become an invalid statistical cliche in this field: " . . . scanning and/or reading (formal communication) was about one-and-a-half times as important as informal communication in the form of discussion. . . ." Omitted from this summary statement are several facts: (1) the scanning and/or reading refers only to journals; other formal sources were left untabulated, (2) discussion refers to discussion at meetings *or* discussion in the home laboratory; the proportion of responses favoring both types of discussion taken together is *greater* than the proportion favoring scanning and/or reading, (3) several other informal sources were left untabulated, including one other discussion category. A most basic objection to this assertion concerns the question that was asked, "Was there any special information you received that influenced you during the course of the research? If YES, how did you learn of it?" No inference as to the general importance of an information source can be founded only on the special information it provides.

The first analysis, involving attributes of the individual scientists, yielded these findings, among others:

(1) Among younger scientists, ideas for current research came almost twice as often from informal discussion as from the technical literature; older scientists drew ideas for current research mainly from the technical literature, by the same ratio.

(2) Each scientist nominated two "most respected scientists" in his field and stated the nature of his contacts with them. Younger scientists were likely to have contact with their "most respected scientists" only through the literature (26 per cent, versus 12 per cent informal contact). Older scientists were more likely to contact their "most respected scientists" personally (18 per cent, versus 8 per cent through the technical literature).

(3) Older scientists were more likely than younger scientists to report no restrictions on travel, to have assistants available, to work alone (i.e., not in a group project),

to have professional duties such as an editorship, to hold several professional memberships, to be absent from the laboratory on professional business, and to visit other laboratories. It is difficult to find support in the data, however, for the authors' assertion that older scientists attended more professional meetings and attended meetings in greater numbers.

(4) Younger scientists mentioned in-laboratory colleagues and visiting scientists equally often as sources of information via discussion. Older scientists mentioned visiting scientists somewhat more and in-laboratory colleagues much less.

(5) Older scientists had developed informal communication networks outside the laboratory to a much greater extent than had younger scientists. For instance, the older scientists received and passed on information to more colleagues outside the laboratory; they were more likely to send out reprints and preprints; they were more likely to have regular mailing lists and their lists were longer.

(6) By three measures of productivity (having presented a paper during the past 12 months, median number of papers presented, and median number of projects completed during the past 5 years), older scientists were much more productive than younger scientists. The authors are careful to observe, however, that

the relationship between extensive informal communication and productivity does not justify a causal inference.

(7) Tabulating the same series of responses by sex, it appears that women were more likely than men to have informal contact with their "most respected scientists." However, the women averaged three years older than the men in the sample, and in any event the number of women in this tabulation was too small for the computation of stable percentages.

(8) Women were less likely than men to have assistants available; fewer of them reported restrictions on travel or long-distance telephoning. Otherwise there appeared to be no sex differences in communication opportunities related to laboratory policy.

(9) Women may have perceived fewer restrictions on travel because in fact they traveled less. They were much less likely than men to visit other laboratories, to hold temporary appointments elsewhere, to attend professional meetings, and to have professional duties.

(10) In their reliance on in-laboratory colleagues for information via informal discussion, women were quite similar to <u>younger</u> scientists as covered in (4) and (5) above.

(11) Women held their own with men in producing articles, but they were considerably behind in other measures of

productivity (e.g., papers presented, 26.5 to 50.7 per cent; books, 4.4 to 14.5 per cent).

The second analysis involved laboratory policies and practices against criteria of productivity and efficiency. Among its findings were:

(12) Unrestricted long-distance telephoning correlates highly with success in obtaining information (one of the efficiency indices) but not with productivity.

(13) Unrestricted travel correlates highly both with productivity and with efficiency.

(14) Payment of expenses to meetings is not a strong correlate of productivity or efficiency. This may be explained by a defective question: "Are your travel expenses to conventions paid only if you participate?" Assuming that there must have been one scientist in the sample whose expenses were not paid even if he did participate, the yes-no response categories were inadequate.

(15) Availability of assistants is uncorrelated with productivity and efficiency.

(16) The use of paid consultants is <u>negatively</u> correlated with productivity and efficiency.

(17) Productivity and efficiency is positively correlated with diversity of research interests in the laboratory (i.e., scientists in the laboratory claiming to be alone in their research interests). Interpretation

of this relationship is difficult. Scientists who pursue research specialties independently may be senior researchers who are more productive and efficient anyway. The authors chose Pelz's interpretation (1956) -- higher productivity given daily contact with dissimilar researchers. But Pelz controlled scientists' rank, and this analysis does not.

(18) Discussion of research with visiting scientists is positively correlated with productivity but uncorrelated with efficiency.

(19) Membership in a discussion group is a strong positive correlate of productivity and a weak positive correlate of efficiency.

(20) Membership in a group project is <u>negatively</u> correlated with (individual) productivity and with efficiency. Controls on age and rank are needed before this relationship can be interpreted, however.

(21) Private university laboratories and public university laboratories ranked first and second, respectively, on each of the productivity measures.

Logical next steps with such data would be a factor analysis of the entire matrix of policy and communication variables and a multiple regression analysis of the combined set of variables against the criterion of productivity. These analyses could be performed (with a somewhat cavalier treatment of statistical assumptions) if the 64 individual laboratories were taken as the units of analysis and their scores on each variable transformed to ranks. Multivariate analyses are so rare in this field that a good body of data should be analyzed up to (and, for heuristic purposes, beyond) the limits imposed by its scales of measurement.

Crane, 1965. This study of the productivity of "scientists at major and minor universities" does not touch upon information inputs at all, but the reader who is aware of the themes and concepts emerging from current information-flow research -- "invisible colleges", intense informal communication, strategically located research centers, freedom and funds, "the affluent scientific commuter" -- may infer that information flow is a factor in such relationships as these:

(1) Scientists affiliated with the major university were much more productive than scientists affiliated with the two minor universities (a "major", a "high minor", and a "low minor" university were included in the study; 72, 36, and 42 scientists from departments of biology, political science, and psychology were interviewed in each).

(2) Scientists who had completed the Ph.D. at a major university and were now affiliated with a minor university were slightly more productive than scientists who had gone from minor to major and much more productive than scientists who had gone from minor to minor (or remained where they were). Most productive, however, were scientists who had completed the Ph.D. at a major

university and remained at that or another major university. An information-flow interpretation of this finding would stress the "invisible college" that the scientist held membership in through his graduate school and his present affiliation.

(3) The prestige of the scientist's sponsor (when he was a graduate student) appeared to affect his productivity independently of the prestige of the graduate school itself. Again it is tempting to interpret this finding in terms of the "invisible college" that the scientist became a member of by virtue of association with his sponsor.

(4) Scientists affiliated with the major university began publishing major work sooner (72 per cent within five years after completing the Ph.D., versus 56 and 43 per cent for the two minor universities).

The author's measure of productivity is an improvement over earlier measures in that it takes account of major and minor publications. A book was given the weight of four journal articles. Joint authorship of a book with more than two other individuals devalued the book to a minor publication. These are still arbitrary operational definitions of productivity differences, however, and this criterion (which will become important as we attempt more often to correlate information inputs with information outputs) should eventually be defined in terms of expended effort, expended time, or a similar self-investment factor.

Hagstrom, 1965. Interviews were conducted with 79 faculty-level scientists and 13 graduate students and technicians associated with them in this sociological study of the ways in which scientists organize themselves, cooperate, compete, engage in disputes, publish their work, etc. The sample was drawn from the "exact" science departments of five universities.

In the next epoch of information-flow research we should begin to make sense of individual differences in communication behavior. Hagstrom presents a typology of scientists that may be useful in talking about idiosyncrasy:

(1) Scientific statesmen. Men with established reputations who have made contributions to their own field in the past and now communicate primarily with specialists in other fields and with nonscientists. Presumably they now have fewer informal contacts within their field than before.

(2) Highly involved leaders. Men who participate a great deal in all the communication channels within the field, both formal and informal. Much of their available time is occupied with travel, meetings, colloquia, professional duties, etc. So much time is given over to communication that they spend little time in research itself.

(3) Informal leaders. Men with many informal contacts but few formal ones. They visit, correspond, and discuss work within their departments, but they avoid the formal

activities of scientific societies. These men tend not to read the literature in their fields. Enrico Fermi is suggested as an example.

(4) Student-oriented leaders. Men who have somewhat less contact with their colleagues but spend a disproportionate amount of time with their students. They often retain contact with former students. Sometimes they are regarded as leaders of "schools", consisting of former and present students, which express their distinctive points of view.

(5) Student-oriented scientists. Less eminent men noted not for their own work but for the work of their students, who are their primary links with the scientific community.

(6) Intradepartmentally oriented scientists. Some men, lacking prestige necessary to approach scientists outside their own departments, rely on departmental colleagues both for communication and for collaboration. In effect they depend on others in the department for assistance in publishing research. Hagstrom thinks this is an unstable type, if the scientist is unable to reciprocate the assistance others have proffered him. The scientist in his sample who fit this type seemed likely to give up research in favor of undergraduate teaching.

(7) Productive isolates. Usually men who are alone in a research specialty within their departments. They are

isolated only in terms of informal discussion; they use formal sources extensively.

(8) Nonproductive isolates. When specialization does not account for isolation, it may just be that the scientist is turning from research to other interests, such as teaching.

(9) Marginal scientists. Men nominally engaged in research who communicate disproportionately with nonscientists. Unlike the "scientific statesmen", who also communicate with nonscientists, men in this group do not have established reputations within their fields. They seem to be serving as consultants or popularizers of their specialties in order to obtain recognition not accorded them within their fields. This tends to be an unstable type; one "marginal scientist" in Hagstrom's sample left the university department and became an employee of the applied science agency where he had been a consultant.

Hagstrom computed correlations (using Yule's Q, an undesirable statistic for this purpose) between productivity and three communication variables: intradepartmental communication, extradepartmental communication, and participation in professional societies. Productivity was correlated most strongly with extradepartmental communication (.85). Correlations between productivity and society participation (.48) and between productivity and intradepartmental communication (.42) may or may not be significant for this small sample; data necessary for computing the standard error of Q are not reported. The obtained correlation of 1.00 between extradepartmental communication and society participation is spurious (in the sense that, if these variables were perfectly correlated, they would have to correlate equally with any third variable, such as productivity), but it is reasonable to expect a high -- if less than perfect -- correlation between them. The two external communication behaviors are correlated .55 and .54 with intra-departmental communication.

Hagstrom raises certain issues concerning "the politics of science" that are treated naively in most use studies. There are important information-flow implications in such topics as competition for recognition, the conduct of disputes, secrecy and simultaneous discovery, etc. Just as there are institutional constraints on the free flow of information (e.g., security classification in defense research), there are likely to be differences between scientists and between research specialties in the political functions served by secrecy and publicity.

Information Flow Research in Progress

The reviewer has not yet seen Current Research and Development in Documentation No. 14 and therefore will not attempt a systematic summary of research in progress. By consulting this source when it becomes available and also the Science Information Exchange of the Smithsonian Institution, the reader can inform himself of almost all ongoing work.

Perhaps it is pertinent to mention here a project in which the reviewer is participating. The Stanford University Institute for Communication Research is undertaking a series of related studies of information flow among communication researchers (Edwin Parker, principal investigator). Communication researchers were chosen as the population of scientists for this project because they represent, almost archetypally, the movement toward interdisciplinary research in the behavioral sciences. Since information channels, even informal ones, are established primarily within disciplines, the problem shared by all interdisciplinary researchers is that of monitoring many discipline-centered systems simultaneously to glean small amounts of relevant information from them. Some communication researchers cultivate specialties within traditional disciplines, however, and they will serve as a comparison group for others who work across disciplines. In addition to studies focusing on the individual scientist, a citation study is providing data on bibliographic coupling among journals in the several disciplines communication research draws upon.

A project that will be influential in guiding future work is Menzel's synthesis of information-flow research (in preparation), which will present findings of his own recent studies and pull together whatever generalizations are supported by the accumulation of data in this field. Menzel's work, when published, will undoubtedly supersede most of this review.

III

TWO DETAILED SUMMARIES

Two studies have been selected for detailed summary. In their strengths and weaknesses and in the general tenor of their findings they are typical of well-conceived efforts in this field.

Menzel's study of information exchange among biochemists, chemists, and zoologists (1958) is summarized first. As a pilot study with a small sample, this work would not merit a detailed summary except for the attention it focused on informal, unplanned episodes in scientific information flow. Its findings have enlarged the compass of subsequent information-flow research.

The second study (or, properly, series of studies) is the American Psychological Association's Project on Scientific Information Exchange in Psychology. This project marks a "first" both in its coordination of many complementary substudies and in its concerted focus on the behavior of <u>behavioral</u> scientists. It is clear already that the challenge to other investigators in the field is to surpass the APA project both in scope and in methodological sophistication.

<u>Information Exchange among Biochemists, Chemists, Zoologists</u>

The Bureau of Applied Social Research at Columbia University undertook in 1957 to study information activities of biochemists, chemists, and zoologists on the faculty of a single academic institution. The study was frankly exploratory; ways were being sought in which communication research by interview survey methods would contribute to an understanding of the needs and means of scientific information exchange.

The goals of the larger study for which this served as a pilot project are:

> (1) To distinguish the types of informational <u>needs</u> which scientists have, and to determine in what respects they remain unsatisfied.
>
> (2) To examine the <u>means</u> and occasions of scientific information exchange, in order to single out the features which make them more or less able to meet the scientist's several needs.
>
> (3) To analyze characteristics of the scientist's speciality, his institution, and his outlook as possible conditions which influence his needs for information, his opportunities for satisfying them, and, hence, his information-gathering habits and felt satisfaction. (Menzel, 1958, p. 4)

While recognizing that any program of action would have to address itself to the <u>means</u> of information exchange, it was felt that the basis for assessing the situation and the starting point for research must be the informational <u>needs</u> of the scientific community. As Menzel stated the priority, "Only when it is understood just what things scientific communication is expected to accomplish will it become possible to investigate to what extent each of these needs remains unsatisfied today, and what means are available for fulfilling it." (1958, p. 4)

Interviews were obtained with 28 biochemists, 28 chemists, and 21 zoologists. These scientists were atypical of any general population of scientists in that their research center is one of the foremost in the country and is located close to other research centers at a crossroads of international traffic. They enjoy rich opportunities for personal contact with others prominent in their fields. Such contacts may encourage greater dependence on the interpersonal information network and less dependence on impersonal sources (journals, books, monographs, etc.) than would be true of scientists more remotely situated.

The analysis of interview responses was organized according to functions served by the scientific communication system, namely: (1) providing scientists with available answers to specific questions, (2) keeping scientists abreast of current developments in their chosen areas of attention, (3) enabling scientists to review recent years' work in an area, (4) giving testimony to the reliability of a source of information, (5) broadening a scientist's area of attention, (6) furnishing the scientist with feedback in the form of responses to his own statements, (7) helping the scientist to orient his work within the totality of research endeavors. A few of the project's findings relative to each of these functions:

(1) Furnishing answers to specific questions. Since the use of indexes, abstracts, and similar reference material had already received much attention, it was decided to concentrate on less obvious

mechanisms by which answers to specific questions were secured. The investigators hoped that such a focus would: (1) illuminate informal avenues of communication to be given more consideration in the planning of communication policy, and (2) reveal services which the official reference facilities fail to perform (as inferred from the fact that scientists turn to informal sources when such services are needed).

The respondents' examples of using channels "other than just the literature" showed an intimate connection between the content of the information sought and reasons for seeking it outside the regular channels of literature search. In two-thirds of the reported cases the information sought was quite unlikely to be found in the literature. Most of these searches were for procedural details (in contrast to theory, data, or conclusions), especially the use of techniques and the adaptation of apparatus. Such methodological details are seldom reported in detail in research articles; even when reported, they are difficult to index and hence difficult to retrieve.

In half the remaining cases (i.e., one-sixth of the total), the information was already available in print and the scientist used interpersonal sources simply because it was easier. (Reviewer's aside: These cases challenge the hypothesis that scientists turn to interpersonal sources <u>to bridge a gap</u> in the literature's coverage. The choice of source, when information is available from more than one, is probably a function of anticipated utility and anticipated cost.)

In the final one-sixth of the reported cases the information subsequently became available in the literature but was not in print when the question was asked. In these cases the scientist, by asking a person associated with the research or a person who had seen early reports, overcame the publication lag in learning of new findings of importance to his work.

Perhaps because of the location of this research center, nearly half the interpersonal inquiries reported by the sample were addressed to the scientists most qualified to answer them. Such sources were authors of relevant publications, developers of instruments or techniques, or recognized leaders in a speciality about which more information was needed. There were even a few cases in which the inquiry reached a person uniquely qualified to answer it, although his expertise was not known by the inquirer, through the agency of a third person who knew the question and knew the person who could answer it.

(2) Keeping scientists abreast of current developments. The project found that these scientists spent great portions of their time and prodigious amounts of effort at "keeping up". They regularly scanned an average of 16.8 scientific journals; 62 per cent of them used at least one abstracting service; 75 per cent regularly read annual review articles; 75 per cent regularly read abstracts of papers given at meetings they did not attend. In addition they processed a great miscellany of non-archival printed materials such as bulletins, newsletters, and correspondence.

The interviewed scientists had attended an average of 2.6 out-of-town meetings and conferences in the year past. They had also attended colloquia and meetings in town, including participation (in 85 per cent of the cases) in seminars and colloquia at their own institution at least weekly during the year.

When the respondents were asked whether they had accidentally obtained some information with "keeping up" value, slightly less than half the sample could recall a recent occasion of this. In about 75 per cent of these cases the information source was another person, a colleague at the respondent's institution or a scientist with whom the respondent happened to be conversing for other reasons. In only 11 per cent of these cases was information obtained from the literature in the course of an unrelated search.

Almost half of the "keeping up" information thus accidentally obtained concerned procedures and apparatus. A smaller percentage concerned new findings or principles, and a still smaller percentage concerned the "who does what" and "where can you get it" questions.

Menzel asks, reflecting on these data, "Why should the accidental manner of learning of new developments be so prevalent?", and then offers a possible explanation (1958, p. 46): "Part of the reason must be sought in the nature of specialization among the basic researchers at the top level. They not only specialize to a high degree, but they delineate their specialties in highly individual and original ways; often no more than a small handful will be specializing in precisely the same area." Since all possible ways of classifying content cannot be taken into consideration in indexing publications, the specialist is bound to overlook items of potential interest to him. His colleagues, whose research interests are sufficiently different to lead them to different parts of the literature, are likely to come across items they suspect to be of interest to him. " . . . if enough members of a given department or research group are plugged into branches of the professional grapevine through consultantships, secondary appointments at other institutions, committee services, and personal correspondence and visits, they may collectively be able to assure each of them a good share of the news about work in progress that interests him."

(3) Providing scientists with reviews of recent years' developments. Sometimes the scientist wishes to inform himself about an area of research somewhat new to him. In such a case he needs to learn of current developments in the context of a decade or more of previous research. Placing current research in historical perspective is the traditional function of review articles, and the respondents were asked therefore what sources they used to "brush up". Surprisingly, review articles were mentioned less often than were primary articles, colleagues, and books. Only theses were mentioned less often as a source of information for "brushing up".

Books were the modal source respondents turned to first. Generally these were advanced textbooks in the field of interest. Since even advanced texts are written primarily for students, and since a text is usually one or more years behind the latest research work by the time it is published, it is puzzling that scientists involved in highly specialized basic research should make so much use of them. The answer to this puzzle seems to be the difficulty of comprehending primary articles in an area somewhat foreign to the scientist's own. The scientist who branches out from his area of specialization becomes a student again, and he uses the texts to familiarize himself with terminology, principles, etc., before moving on -- as almost all the respondents did -- to review articles, primary articles, and abstracts. Similarly, few respondents consulted colleagues first, but several turned to colleagues secondly, after obtaining an overview of the field from a textbook.

(4) Certifying the reliability of a source of information. There appear to be three reasons why a scientist sometimes wishes to check the reliability of information he encounters even in primary journals: (a) the ever-increasing volume of scientific output, (b) the proliferation of specialties, and (c) the decline of traditional forms of public critique of other scientists' work. The first two reasons imply that the scientist will notice an increasing number of new names and unfamiliar topics. The creation of new journals to deal with this growth aggravates the reliability problem if the scientist does not fully trust the judgment of the editors of these journals. The decline of traditional forms of public critique refers to the ratio of critical review articles to original publications deserving critique. A biochemist in Menzel's sample noted that there are now "fewer critical reviews and fewer critical arguments," both of

which he wished to have to evaluate work in other fields. A zoologist in the sample deplored the politeness and courtesy with which papers are received at American scientific meetings (he felt that the tenor of such meetings used to be different in this country and still is different in Europe). He would prefer to hear highly critical comments from the floor at paper-reading sessions, since such comments would help him to assess the significance of presented papers.

These traditional critical mechanisms having failed the scientist, it was found that he now relies largely on the judgment of his colleagues when the reliability of a source of information is in question. Such a finding particularly reflects the stature of the research center Menzel studied; it presupposes that the scientist can conveniently query a number of colleagues whose judgment he respects. How a scientist checks reliability at a smaller and more remote research center remains to be determined.

(5) <u>Broadening a scientist's area of attention</u>. Scientific curiosity has a way of opening up new areas of interest for its possessor. When this happens the scientist seeks to inform himself more systematically of the substance of the new area. Menzel asked each scientist to relate how he had become interested in a (recently discovered) new area of research and how he had informed himself about it.

Curiosity is not the only motive for beginning to follow developments in a previously nonsalient area of research. Sometimes a research project leads the scientist into new areas because of unforeseen problems or findings. Sometimes decisions are made at an institutional level that redirect the efforts and attention of affiliated scientists. Scientists working partly or wholly in applied research may be forced into new areas because of demands made by the consumers of their research. In addition there are departmental colleagues seeking collaboration, outside colleagues seeking consultation, and students seeking advice -- all of whom may motivate the scientist to learn more about a new area.

Except anecdotally, little was learned of the ways in which scientists explore a new area after becoming interested in it. One chemist read (by his own estimate) 600 articles dealing with a single element after deciding that the element was worth investigating. Another began to read a section of Chemical Abstracts that he had previously ignored. In general a scientist whose interest was aroused began the "brushing up" information search outlined in (3) above.

(6) Furnishing the scientist with feedback to his own statements. Reactions of colleagues to a scientist's work will sometimes include the helpful criticism that enables him to carry the work forward. Both the interpersonal and the impersonal communication systems bring back to the scientist such reactions to statements he advances, in oral or written form. Menzel records three general types of feedback processes: (1) others raise questions after they have heard or read the scientist's statement; (2) others present criticisms of the scientist's work and point out shortcomings or additional problems;

(3) full working discussion is carried on between the originator and the others.

It appears from Menzel's data that face-to-face feedback is the modal form (noting again, however, that in this research center the opportunity for conversation with respected and knowledgeable others is unusually good). The feedback comes from colleagues, visitors seeking consultation, and students. One chemist even reported a useful discussion in the freshman chemistry class he was teaching: "Some kids last term pounded and pounded, and I was forced to get down to fundamentals to explain to them what it was. I had never thought so deeply about the concept before."

Similar vigorous give-and-take was reported by some of the sample as a virtue of conferences, particularly the smaller meetings. They cited instances in which papers they had presented had been criticized both constructively and destructively, with consequent effects on their decisions to continue on projects with and without modification.

A side benefit of such discussions was reported by some scientists. They found that their thinking about an issue became clearer as they listened to themselves talking about it, or even as they rehearsed a statement subvocally.

(7) Helping the scientist to orient his work within the totality of research endeavors. Scientists also depend upon communication to compare the significance of their work (and of contemplated future work) with the significance of all other research being conducted

in their own and adjunct fields. Engrossed in his work and intrigued by the problems it poses, the scientist is in danger of losing perspective. Several scientists in Menzel's sample mentioned that conferences gave them an opportunity to note "the relative importance which the group as a whole attaches to particular topics of research." Presumably such gatherings serve this particular assessment function better than review articles and "state of the art" papers, since the latter overviews are likely to be the work of one man or, at the most, several men -- therefore not indicative of the consensus.

Data on use of specific information channels. In addition to the functional analysis summarized above, the data collected by Menzel and his staff were analyzed in terms of over-all use made of information channels (specifically, primary journal articles, review articles, abstracts, and scientific meetings).

The mean number of primary journals scanned by chemists in the sample was twelve; by biochemists, thirteen; and by zoologists, thirty. The same differences in spread of attention over many journals were noted in response to the question, "About what fraction of the articles you usually read appears in (the three journals you regard as most important for your work)?" The chemists, biochemists, and zoologists reported that fraction to be 64, 56, and 24 per cent, respectively. There was also much less agreement among zoologists than among the other two groups in nominations of most important journals in their field.

The sample voiced a number of complaints about the journals they used. Some felt that the quality of published research was decreasing even as its quantity was increasing. Many complained of the time lag between submission and publication. The necessity of brevity in description of procedures, apparatus, etc. -- the details often most desired by readers -- was mentioned as a shortcoming. One scientist regretted that journals seldom publish negative results, information that would identify a fruitless approach as a warning to others who might be considering it.

The use of reviews (annual reviews, review articles) by the sample was widespread even though many defects of this form of communication were pointed out. About three-quarters of all the scientists regularly read one or more annual reviews, and reviews were frequently mentioned among the four most important channels for keeping abreast in a scientist's primary field (dependence on reviews in keeping up with secondary fields seemed to be even greater).

The general criticisms of reviews concerned publication delays, failure of the reviewer to exercise critical judgment, to synthesize and interpret, and to make his specialty comprehensible to readers in other specialties. Some scientists complained that reviews are not comprehensive, either spotty in their coverage of present research or insufficient in establishing continuity from past to present work.

Abstracts and indexes are scanned regularly by about two-thirds of the sample, with no observable differences in use attributable to the scientist's discipline. In general abstracts were scanned _after_ the scientist had scanned his usual quota of primary journals; he apparently sought to make sure that nothing had been overlooked in sources he could not scan directly. It is suggested that abstract-scanning does not supplant the direct inspection of journals because of long abstracting time lags.

The print channels used by this sample of scientists are almost wholly published in English. The use of non-English journals, for instance, makes up only 10 per cent of all journal use across disciplines. Yet these scientists name foreign institutions about 35 per cent of the time when asked which institutions are "most significant" in their fields. The failure to read non-English journals cannot be attributed entirely to language barriers, since almost all scientists in the sample could read both French and German. Russian was the only language frequently mentioned as an obstacle to keeping up with the literature.

Four-fifths of the interviewed scientists had attended meetings of scientific societies during the year preceding the interviews. Yet two-thirds of the sample judged that listening to papers at meetings seldom pays off. What continued to draw these scientists to meetings, apparently, were the special program events (such as symposia) and the opportunity for informal contact with other scientists in their fields. If the "elbow-rubbing" function of meetings was endorsed even by scientists at this large and strategically located research center, it may be hypothesized that scientists more remotely situated would value meetings still more for this reason.

<u>Aftermath of the Menzel study</u>. Perhaps it is a good sign that this pilot study raised more questions than it answered. Menzel himself has enumerated these questions in subsequent articles (1959, 1964) and in his review of use studies for the National Science Foundation (1960). One set of questions concerns the functions served by scientific information: Can an exhaustive set of functions be specified? Can every information-seeking act be explained in terms of one or more functions served by the information? Another set of questions concerns alternate channels of information flow: Do the different channels (especially the impersonal and the interpersonal) carry different kinds of information? Are there factors in the research environment that lead to different proportions of use of these channels? Other questions concern the deliberateness of the information-acquisition act: How much information comes unsought? How important does unsought information prove to be? There are also questions of effectiveness raised for reasons of policy: What information functions are adequately served by the combination of channels normally available to a scientist? Should new channels be opened? Should existing channels be modified? Should an effort be made to systematize the accidental acquisition of information? Several of these questions figure prominently in Menzel's later research.

Positive response to the Menzel study is implicit in subsequent citation and borrowing of ideas. Some negative response has also appeared. Shaw (1959) criticizes the small sample, doubts that any facts were uncovered that were not already common knowledge in the field, and challenges the fundamental premise that scientists (the users) are qualified to evaluate such documentation services as review articles. In support of his view, Shaw notes the small number of scientists in the sample who were able to offer a pertinent suggestion for improving review articles. Much the same point was made by Taube in his evaluation of use studies (including Menzel's) presented at the International Conference on Scientific Information (1958). He said that documentation is a professional service, not a consumer service, and that user acceptance was no more valid a criterion in documentation than in medicine, where standards of practice are not established by patients' opinions. This issue concerns the application of use study findings, however, not the empirical study of information flow itself.

It may be said that Menzel's study marked the end of the beginning of research on scientific information flow. Together with other studies of the mid-fifties (e.g., Herner, 1954; Glass and Norwood, 1959), it emphasized the importance of interpersonal information sources, showed low percentages of use of many esteemed formal systems, and revealed the role that happenstance plays in the acquisition of information. Studies from the middle epoch of research in scientific information (in which we find ourselves) typically take account of these factors.

Information Exchange among Psychologists

In the fall of 1961 the American Psychological Association began a series of studies to trace patterns of information exchange among psychologists. Thus far twelve studies have been published as Reports of the American Psychological Association's Project on Scienti·· Information Exchange in Psychology (1963-). Other articles by the project's directors (W.D. Garvey and B.C. Griffith) have appeared, and reports of other related studies have been circulated in mimeographed form (e.g., Jakobovits and Osgood, 1963; Xhignesse and Osgood, 1963).

The twelve Reports will be summarized separately:

APA-PSIEP Report #1. Scientific activity and information problems of selected psychologists. As an initial effort in the Project on Scientific Information Exchange in Psychology, the information activities of a selected group of researchers were studied by means of detailed logs kept over a two-week period. The sample of log-keepers consisted of every twentieth author cited in each chapter of the Annual Review of Psychology beginning with the most recent year and using volumes for previous years until all areas of the Annual Review were represented. Of 132 authors whose cooperation was solicited, 78, or 59 per cent, returned useable logs. These psychologists ranged in age from 30 to 65 and tended to hold senior academic positions (the modal rank was full professor).

One of the most striking findings of the log study was the amount of information disseminated orally in symposia, colloquia, seminars, research conferences, etc. A type of researcher identified as an "information man" attended a large number of such meetings and relayed his observations to colleagues and students at his own institution. Slightly more than 25 per cent of the sample could be characterized as "information men".

Personal correspondence also proved to be an important means of sharing information with investigators at work on similar projects. In addition to the constant exchange of preprints and reprints, letters were written requesting information and answering requests from other investigators. Still other correspondence discussed the scheduling of colloquia, the preparation of programs, etc.

If the 78 log-keepers may be taken as a representative sample, American psychologists tend to correspond with each other and not with psychologists in other countries. Correspondence with persons in other countries was limited largely to the exchange of reprints, and that only rarely.

The amount of time spent reading by the log-keepers in the two-week period ranged from zero to fifty hours, with a median of seven hours. The 61 respondents who completed a follow-up questionnaire on their reading habits mentioned 32 journals, of which only 12 were cited more than once. The modal date of "last article read" fell in the year of the study (1962), while the median date fell in the year before. Articles published in those two years accounted for two-thirds of the articles most recently read by the sample.

Finally, the log-keepers reported on their use of Psychological Abstracts. Forty-four of the researchers had made recent use of the Abstracts for one or more of the following purposes: to seek specific information (34), to maintain knowledge of a field (20), and to facilitate a literature search (15). In addition to the Abstracts, 26 other indices and abstracting services were mentioned. Such diversity of information sources is not surprising in view of the fact that researchers in the sample mentioned using publications from 35 fields other than psychology (e.g., acoustics, administrative science, aesthetics, anatomy, anthropology).

APA-PSIEP Report #2. An informal study of the preparation of chapters for the Annual Review of Psychology. The preparation of an Annual Review of Psychology chapter is a major information-processing task, typically involving reference to two hundred or more articles, technical reports, and books published within the period being reviewed. Because the literature cited must be current, abstracts and indices are of slight help. Because the reviewer is expected to lead the reader to available printed materials, the informal interpersonal network from which he gains much of his own information (cf. Menzel, 1958) cannot be fully acknowledged.

Of 128 reviewers who had prepared Annual Review chapters during the years 1956 through 1962, 81, or 63 per cent, replied to inquiries concerning their information-processing activities, their attitudes and objectives as reviewers, and their perceptions of inadequacies in information retrieval services available to them.

Almost all reviewers (91 per cent) depended upon an issue-by-issue inspection of journals known to be relevant to the topic of the review. Almost half (46 per cent) covered peripherally relevant journals in the same manner (note that this difference may be a function only of each reviewer's definition of "relevant" and "possibly relevant" journals). The third most common procedure (34 per cent of the 81 reviewers) was a scanning of abstracts and indices. Only six of the reviewers mentioned conversations with colleagues as a source of information, although the converse of this finding, that the great majority of reviewers did not obtain information for the review through conversations with colleagues, is hard to believe.

APA-PSIEP Report #3. A general study of the annual convention of the American Psychological Association. This comparison of the 1936, 1951, 1957, and 1961 conventions of the APA focuses on structural factors such as the number of papers and symposia placed on the program by each division, the percentage of submitted papers and symposia rejected by each division, and experience and educational background of persons presenting papers and symposia. Findings which suggest a changing pattern of information exchange include: (1) a ten-fold increase from 1936 to 1961 in the number of events scheduled, (2) a greater proportion of participants in the 1961 convention not affiliated with colleges and universities, (3) an increase in the number of graduate student participants, (4) a trend toward brief research reports presented by younger persons and symposia presented by older and more experienced psychologists, (5) fairly high rejection rates for papers and symposia submitted to the three later conventions versus almost total acceptance in 1936 of papers meeting minimum quality standards, (6) highly variable rejection rates by division in later conventions (from zero up to fifty per cent or higher).

No evidence is found for the contention that earlier conventions were more effective in scientific information exchange, although "sheer size cannot be eliminated as a factor that may reduce the perceived effectiveness of meetings."

APA-PSIEP Report #4. Convention attendants and their use of the convention as a source of scientific information. Beginning with the premise that "conventions serve as an important channel for rapid and immediate exchange of scientific information", four meetings of psychologists were studied in 1962 to determine "the characteristics of attendants, their use of the convention to obtain information, and the function and characteristics of programmed and informal events as sources of information for attendants." In addition to the national APA convention, meetings of the Rocky Mountain Psychological Association, the Eastern Psychological Association, and the Psychonomic Society (a group of experimental psychologists) were included in the study. Questionnaires were sent to samples of attendants at the meetings (except that questionnaires were sent to every Psychonomic Society attendant whose address could be found). Questionnaire mailings were timed such that attendants would receive them immediately after returning from the meetings, while recall of the experience was freshest.

There is evidence that attendants used the four meetings differently (e.g., 45 per cent of the Psychonomic Society sample, versus only 28 per cent of the national APA sample, sought specific information at informal events, presumably because the smaller and more select group allows members to be more extensively acquainted with each other and aware of each other's areas of expertise), but experiences at the national APA meeting, for which the sample of respondents is largest (10 per cent sample, 409 questionnaires mailed, 277 useable questionnaires returned), may be summarized as representative of all four meetings.

The APA respondents said that clinical work, research, and teaching consumed the greatest amount of their working time, in that order. When asked what activity required the greatest effort to gather and use scientific information, however, nearly half of the respondents (47 per cent) replied research -- clinical work and teaching each being mentioned by only 16 per cent of the sample.

Three subject areas within psychology -- statistics and measurement, testing, and personality dynamics -- were ranked first or second by more than half the respondents in reporting their information needs. Five subject areas outside of psychology -- sociology, education, anthropology, psychiatry, and physiology -- were ranked first or second in information need by at least a quarter of the sample.

Respondents seeking specific information at the APA meeting turned to symposia, informal events, paper presentations, and exhibits (of apparatus, books, etc.), in that order (36, 28, 26, and 19 per cent of the sample). Specific information sought most often concerned problems of method, procedure, or apparatus (cf. Menzel, 1964 -- interpersonal communication among scientists informs them about "apparatus and procedures, 'know-how' information that seldom finds its way into the literature, let alone into the indexes."). Of the attendants seeking specific information, 71 per cent said they had obtained it, 15 per cent said they had been partially successful, and 14 per cent said they had failed entirely to obtain the information.

Respondents were also asked about the most significant pieces of information (specifically sought or not) they had obtained at the convention. Such most-significant information was said to pertain to methodology, theory, conclusions, data, and statistics by 39, 32, 29, 17, and 4 per cent of the sample, respectively. Symposia and informal discussions were the modal sources of such information, each mentioned by 35 per cent of the sample, while contributed papers and invited addresses followed far behind with 13 and 5 per cent.

Considering the preponderance of time given over to contributed papers on the APA program, it is disturbing to learn that 87 per cent of the sample found information of greater significance in other program events. It is true that informal discussions are ubiquitous during a convention and that symposia tend to be better attended than paper sessions, but it is not clear whether the 87 per cent listened in vain for significant information at paper sessions or simply failed to attend such sessions at all.

<u>APA-PSIEP Report #5. Convention participants and the dissemination of information at scientific meetings</u>. Persons who made presentations at the 1962 meetings of the Eastern Psychological Association, the American Psychological Association, and the Psychonomic Society were polled by mail in order to: (1) determine whether the three types of meetings -- regional, national, and select -- have different roles in scientific information exchange, (2) locate the convention presentation in the series of events from the inception of a research project to its final reporting in the archives and to determine the timing of the series, (3) locate other methods of dissemination of convention presentations and their timing, (4) determine the result of the presentation and any feedback therefrom on the author's subsequent work, (5) determine the effect of the presentation on further dissemination.

Nearly all participants at the EPA and PS meetings were included in the study, and 20 per cent of the APA meeting participants were sampled by taking every fifth name in the 1962 APA program. With useable return rates ranging from 77 to 86 per cent, sample sizes were, respectively, 262, 102, and 189.

The three types of meetings appear to have different roles in scientific information exchange to the extent that they emphasize different subject areas (i.e., the Psychonomic Society emphasizes physiological and experimental psychology; the EPA is also oriented toward experimental psychology but clinical and social psychology events are programmed as well; the APA provides a broad coverage of general psychology). Attendants at the three meetings might expect, therefore, to obtain different kinds of information.

The 1962 convention presentations reported work begun, on the average, 18 months before the time of the convention. At the APA convention, in particular, symposia presentations frequently reported work under way four years or more. When the work had reached a reportable phase six months or more before the time of the convention, it was more likely than not to have been reported (typically in a colloquium) to an earlier audience. A smaller number of convention presentations had already been reported in written form, usually as theses or dissertations.

A majority of participants planned publication of their presentations, typically journal publication of papers and book publication of symposia. Altogether 80 journals were mentioned by participants of the three meetings as intended outlets.

The convention provides a setting in which information and opinion can flow two ways, from author to audience and back again. Almost all participants reported some post-presentation discussion of their papers, sometimes during the session itself, more often following the session. More than half the participants reported a discussion of their papers with a person who had <u>not</u> heard the presentation. As a result of all discussions, about a third of the participants reported some modification in their plans for publishing papers, designing subsequent research, etc.

From 90 to 99 per cent of participants at various APA sessions received requests for copies of their papers, the median number of requests ranging by session from 6 to 15. More requests were received after the meetings than during the meetings, and many reprints were requested after the convention was over. The program regularly published in the August issue of the American Psychologist may have stimulated requests from persons not attending the convention.

APA-PSIEP Report #6. The publication fate of formal presentations at the 1957 convention of the American Psychological Association. This study, based on responses of 764 participants at the 1957 APA convention (which had taken place five years prior to the time of the investigation), sought to answer four questions: (1) What percentage of presentations made at an annual APA meeting receive journal publication? (2) What is the time-table of submission and publication of these relative to the time the presentation is made? (3) To which journals are these presentations submitted and in which are they finally published? (4) What are the reasons why some presentations are never submitted and published in archival form?

Of the 764 papers and symposia presentations covered in the sample, 375 eventually received publication in archival journals, while 389 did not. Only 22 of the 389 unpublished presentations had been submitted for publication, although 43 were to be submitted "in the near future" (i.e., five years after the convention). Reasons given for the delay in submission included a need for additional data or controls, interruptions and delays in the research program or the writing of the article, and the relationship of the reported research to a still ongoing project.

The time-table of journal publication showed a positively skewed distribution of papers published within five years after the convention. The largest number of papers were published in the period from six months to one year after the convention. Within a year and a half after the convention more than 60 per cent of all published papers had appeared.

The 375 published papers appeared in 89 journals, of which only a minority could be considered primarily psychological in content. The modal number of presentations appearing in a given journal was one; 42 journals published just one article from the 1957 program. The curve was steep: the ten most-used journals carried nearly half (174) of all published presentations. The first two journals, Journal of Abnormal and Social Psychology and Journal of Experimental Psychology, carried more than one-sixth of all published presentations.

Most articles were accepted by the journal of first submission, but 60 eventually published articles were not published in the journal of first submission for various reasons, the most common being editorial rejection of some aspect of the article's content. The second major reason for non-publication in a journal of first submission was an author's decision not to make requested revisions or not to accept long publication delay.

Journal publication had not been sought and was still not being sought for 352 of the 764 presentations in the 1957 sample. Some 430 reasons for not seeking publication were advanced (not all different, of course). The most frequently advanced reason was that the presentation had been prepared specifically for the APA convention with no thought toward later publication (37 per cent of those giving a reason for failure to seek publication). Second most frequent was the assertion that the presented information was sufficiently available in another form -- in a book, technical report, dissertation, etc. (31 per cent). Third and fourth reasons (19 and 12 per cent) were that the work was part of a yet-uncompleted long-term project and that the reported results were not considered worth the time and effort needed to prepare a journal article.

Three conclusions were drawn from the study. First, about half the presentations became part of the archival literature and thus appeared twice in the information exchange flow and structure. Secondly, nearly all attempts to publish the contents of presentations were successful. Thirdly, those presentations not submitted for publication were for the most part never intended for publication.

<u>APA-PSIEP Report #7. Archival journal articles: their authors and the processes involved in their production</u>. The authors of 396 articles in 25 journals related to psychology were polled by questionnaire to determine: (1) some of the processes and associated time intervals involved in the writing of an article, (2) the author's experiences in submitting articles for publication. A majority of the

articles had more than one author; in these 213 cases a questionnaire was sent to each of the authors and a summary set of responses was compiled for each article.

Among the 691 authors of the 396 articles, 410 were members of the APA. When tabulated by membership level, there proved to be twice as many APA Fellows among the journal authors (proportionately) as in the APA membership at large. The proportion of Members and Associates was lower among the journal authors than in the larger membership. The proportion of doctorate-holders among the authors was about equal to that among the APA membership. Two-thirds of the authors were associated with academic institutions, whereas the 1962 National Register of Scientific and Technical Personnel showed only one-third of American psychologists at work in academic institutions.

When an article was the joint product of two or more authors, certain tasks seem usually to have been assumed by the senior author (arbitrarily defined as the first listed author), and other tasks were assumed by the junior author. Senior authors were very likely to have formulated the research problem, to have designed the experiment, and to have written first, final, and revised drafts of the article. Junior authors were more likely to have conducted the experiment, collected data, and scored data. Senior and junior authors were equally likely to have participated in the statistical analysis.

On the average, work reported in the articles was begun from 30 to 36 months before the date of publication (this may be compared with the 18-month lag between inception and convention presentation reported in Report #5). About one-third of the work had reached a reportable stage two years or more prior to publication. The median time at which work reached a reportable stage was about 19 months prior to publication. Among the work considered reportable two years or more before publication there was a large proportion of thesis research; the delay in these cases is explained by the student's need to complete the thesis before writing for journal submission.

The main contents of 43 per cent of the articles had been reported orally at least once prior to publication. The most frequent occasion for such reports was a national convention of the APA, at which 20 per cent of the articles were presented.

Oral dissemination was supplemented by the distribution of preprints in 36 per cent of the sample. The number of preprints distributed ranged from 1 to more than 200, with a median of 9. Relatively few preprints were requested; most were sent spontaneously to personal friends and colleagues known to be interested in the subject matter.

Virtually all authors distributed additional copies of the article in reprint form. The number of reprints distributed ranged from 1 to 300, with a median of about 26. Almost all articles (94 per cent) prompted specific requests for reprints, and in 62 per cent of the cases reprints were routinely sent to each person on a reprint mailing list.

Authors selected journals in which their articles should appear chiefly on the basis of familiarity with editorial policies and readership (58 per cent of all articles). The appropriateness of the subject matter to a journal and the promise of rapid distribution were each mentioned by 20 per cent of the sample (multiple reasons were allowed; 581 reasons for journal selection were offered for the 371 articles). In 18 per cent of the cases the journal of publication was a second or third choice.

About half of the authors received comments from readers of the published articles. The effect of this feedback on the author's work was minor: only 5 per cent of the authors reported that the comments would have induced them to revise the article if received in time and only 6 per cent reported that the course of their future work would be affected in any way by the comments.

Problems that impeded research reported in the article were mentioned by 40 per cent of the authors. The three most pressing problems concerned information-processing: access to current, unpublished work; access to published reports of limited circulation; use of present, inefficient, indexing services.

APA-PSIEP Report #8. A comparison of scientific information activities at three levels of psychological meetings. The annual meetings of ten state psychological associations and the joint meeting of a regional association and a state association were studied to obtain further data on the role of such meetings in facilitating scientific information exchange. Data from earlier project reports (#4 and #5) were incorporated in the analysis so that differences in the functions of state, regional, and national meetings could be discerned.

In considering the information exchange function of conventions, two questions were explored: (1) Did attendants bring specific problems to the convention in the hope of finding the information? (2) Did attendance furnish them with any information that could be expected to exert a significant effect on their work activities?

For each type of meeting slightly more than half the attendants were in search of specific information. At the state meetings attendants were more often seeking professional (clinical) information, while at the larger meetings they more often sought scientific (research) information.

The data revealed that fewer attendants at the smaller meetings obtained significant information than did attendants at the larger meetings, contrary to the common impression that large meetings are poor events for scientific communication. Contributed papers are a more frequently mentioned source of significant information at the state meetings, while at the regional meetings papers and symposia are mentioned equally often and at the national meeting symposia are mentioned three times as often as contributed papers.

As was true at the national level, the main contents of many of the presentations made at the state meetings had been disseminated in some form prior to the meeting, 30 per cent in oral reports and

23 percent in written reports. Corresponding percentages at the national meeting were 37 per cent and 29 per cent.

Journal publications of the contents of their presentations was planned by 35 per cent of the state participants, 65 per cent of the regional-state participants, and 70 per cent of the national participants. An average of 5 per cent of the participants at the state and regional-state meetings planned to present their work subsequently at the national APA convention.

APA-PSIEP Report #9. The use of scientific journals by psychologists and the readership of current journal articles. This study examined the audience for various psychology-related journals among members of the APA. A 10 per cent sample of APA members and student affiliates was polled by mail questionnaires; 1187 of the 2140 questionnaires were returned and found useable.

The study addressed itself to a large number of questions concerning the use of journals in scientific information exchange. Among the data bearing upon the most significant of these questions:

What percentage of the sample regularly uses each of the 27 journals selected for study? From 91 per cent (American Psychologist) to 2 per cent (J. opt. Soc. Amer.). American Psychologist, because of its automatic distribution to APA members, has almost twice the readership of the next half dozen most regularly read journals -- Psychol. Bull., J. abnorm. soc. Psychol., Psychol. Abstr., Psychol. Rev., J. consult. Psychol., and J. appl. Psychol. (48, 47, 45, 43, 40, and 22 per cent respectively). Journals with interdisciplinary content (e.g., Child Development, Behavioral Science) are regularly read by about 10 per cent of the sample.

What is the relationship between regular use of a journal and other transactions with the journal such as subscribing to it and publishing an article in it? There is a high but unspecified correlation between regularly using a journal and subscribing to it. About 65 per cent of the regular users of an APA journal and about 40 per cent of the regular users of a non-APA journal are subscribers. There was no discernible relationship between the experience of having published in a journal and present regular use of it. Only 4 to 5 per cent of the sample had published in such high-readership journals as American Psychologist, Psychol. Bull., and Psychol. Rev., while 6 to 8 per cent had published in each of the less-read journals J.exp. Psychol., J. comp. physiol. Psychol., J. clin. Psychol., and Amer. J. Psychol.

Do people who use one journal reliably use or fail to use another? Correlational analysis showed that groups of journals are used jointly and that users of journals in one group (e.g., the animal-human experimental group) are not very likely to be users of another group (e.g., the clinical-counseling professional group). When respondents' areas of ongoing research were correlated with journal use and the total matrix factor analyzed, three general factors and many specific factors were obtained, including some couplet factors relating a single area of research and use of a single journal (e.g., physiological research and the Journal of Comparative and Physiological Psychology were the only variables with high loadings on the fifteenth factor).

What are the characteristics of users of different journals or groups of journals? When respondents were classified according to educational background, occupation, area of specialization with psychology, and other attributes, journals and groups of journals were shown to have different kinds of users. For example, psychologists in private industry were quite likely to read J. appl. Psychol. and Personnel Psychol.; they were quite unlikely to read J. Personality or J. clin. Psychol. For another example, social psychologists were much more likely than developmental psychologists to use Behavioral Science; the converse is true of J. Gerontology. These data support the common-sense hypothesis that a journal is used by a psychologist when his occupation and area of specialization arouse his interest in its subject matter and when his education equips him to make use of it (the readership of such journals as Psychometrika and Educational and Psychological Measurement, among others, seems to be contingent on education).

About how many people read any given article in a current issue of a journal? Of the 429 items listed in the table of contents of the current issues of 25 journals, nearly one-half (207) had been read by fewer than 1 per cent of the subsamples of respondents (ranging from 218 to 254 in size) questioned about their readership of specific journals. No research report seems to have been read by more than 6 per cent of the subsample. Only eleven articles of any type were read by more than 10 per cent of the subsample, and ten of these articles appeared in the American Psychologist (the other appeared in Psychol. Bull.). Review and theory articles in the American

Psychologist, Psychol. Rev., and Psychol. Bull. were the most widely read, indicating current interest in such content. At the other extreme, four articles in Psychometrika were read by no persons in the subsample, eight Psychometrika articles were read by one person each, and only one article in that journal had as many as three readers (slightly more than 1 per cent of a subsample).

What events lead to the reading of an article? Most reading acts stem directly from a scanning of the table of contents, not by referral from an index or other source. Most frequently the reader is attracted by the title of an article, not by the author's name or by his institutional affiliation.

What is the article's relevance for the reader's work? In half of the articles read some utility was perceived in the conclusion or in the article's general point. More specific types of information, concerning theory, method, or data, were useful to readers in 18 to 26 per cent of the articles read. In the majority of cases information was stored simply in memory, not in notes or abstracts. Readers mentioned to colleagues some aspect of 13 per cent of the articles they read.

Is there any relationship between a psychologist's professional activities and the number of articles he reads? Only 352 of the 1187 respondents actually read an article in the current issues of the 25 journals. The average number of articles read per reader was 6.04 (or 1.79 when divided by the entire sample). Considering only Ph.D.s among the readers (since amount of education affects readership), more articles were read by psychologists specializing in personality and abnormal psychology, fewer by those in experimental, developmental, and personnel psychology, and fewest by those in educational psychology. Those conducting research in hospitals, private practices, residential institutions, or in the employ of the federal government tended to read more articles than those conducting research in academic institutions. The small number of articles read by psychologists in academic institutions seems anomalous, since these psychologists contribute disproportionately to the programs of conventions and the contents of journals. It is recognized that these readership differences ought to be qualified in terms of readers' scientific productivity, for which data were not available.

APA-PSIEP Report #10. A preliminary study of information exchange activities of foreign psychologists and a comparison of such activities with those occurring in the United States. A questionnaire was sent to all psychologists who made presentations at the 17th International Congress of Psychology (with the exception of eleven U.S. psychologists who had already taken part in a variety of PSIEP studies). Data were sought on the sources of information upon which foreign psychologists depended, on the abstracting and reference services they utilized, and on periodicals important to their work. Useable questionnaires were returned by 93 of the 125 foreign psychologists and by 73 of the 91 U.S. psychologists who had participated in the Congress.

To state the conclusion first: "information exchange activities followed similar patterns among research-oriented psychologists throughout the world." The principal sources of information utilized both by U.S. psychologists and by foreign psychologists were the Annual Review of Psychology, Psychol. Bull., Psychol. Abstr., and Contemporary Psychol. Dependence on these sources was actually greater among the foreign psychologists. Subscription patterns were quite different for the two groups: personal subscriptions in the U.S., institutional subscriptions abroad.

Both groups ranked the usefulness of sources equivalently: first, U.S. journals and books; then discussions within and outside one's employing institution; then correspondence and the exchange of reprints. A psychologist's own work, however, was usually reported in the journals of his own country.

Psychologists of eight world regions differed to some extent in several information activities. Regional patterns are too complex to be summarized here, but the variables studied included: (1) the type of study being reported at the Congress (empirical research, review, theoretical paper, etc.), (2) time elapsed between the inception of the work and its presentation at the Congress, (3) percentage of work receiving oral or written presentation prior to the Congress, (4) occasions for presenting work prior to the Congress, (5) percentage of psychologists in each region planning publication subsequent to the Congress, (6) types of publication planned subsequent to the Congress (journal article, book, monograph, etc.). Regional differences in these variables may be related to differing opportunities and occasions for information exchange and perhaps also to regional traditions in the conduct and reporting of research.

APA-PSIEP Report #11. The discovery and dissemination of scientific information among psychologists in two research environments. The information-exchange activities of psychologists at the University of Minnesota and at the Laboratory of Psychology of the National Institute of Mental Health in Bethesda, Maryland, were studied by means of questionnaires and tape-recorded personal interviews. The investigators sought to determine in what ways the two research environments (academic department and government laboratory) dispose researchers toward different patterns of research planning, information intake, communication with other scientists, etc. The Minnesota sample included 63 psychologists, all but 8 of whom were devoting 20 per cent of their time to research. The Maryland sample included 30 psychologists, all involved in research.

The two research centers were not chosen to be typical. Indeed, the investigators found that the progress of research at Minnesota and at the Laboratory of Psychology was remarkably unhampered by limitations in funding, choice of subject matter, available time, supportive staff, libraries, etc. Therefore studies of less-favored research environments may also be necessary.

At both centers the flow of informal communication about ongoing research was intense and continuous. In addition to opportunities for conversation with colleagues who are at least knowledgeable about, if not directly engaged in, a psychologist's research area, the Minnesota group benefits from the involvement of graduate students in its projects (the feedback they provide, the staff discussions stimulated by thesis committee meetings, etc.) and the Maryland group benefits from the number of distinguished psychologists and other scientists who visit the Laboratory on their trips to Washington.

At both centers there were a few "loners", psychologists who did not discuss their ongoing research either because it was too specialized to be discussable or because they simply preferred to pursue their plans without advice. The possibility was raised that a good research idea might be pirated, intentionally or not, if other psychologists were informed about it.

A slightly larger percentage of the Laboratory sample (97 per cent) had attended some type of meeting or convention during the past year than had the Minnesota sample (89 per cent), and a larger percentage of the Laboratory psychologists had made presentations at such gatherings. However, the meetings attended by the Minnesota sample were more numerous and varied and this group attended, on the average, more meetings per person. Intramural meetings were common at both institutions and were valued for the interaction and information exchange they provided.

Correspondence, other than the exchange of papers, was largely dependent on individual inclinations. For the most part, less time-consuming and more rewarding channels were preferred.

Both groups had evolved or improvised means of dealing with the flood of information in print. In both there was a tendency to depend upon informal, oral leads to relevant archival publications. In some cases "periodical pools" and "journal seminars" gave participating psychologists a chance to divide among themselves the labor of scanning a large number of relevant journals. Each participant, knowing at least the boundaries of others' research interests, would mention related articles he had seen in the journals he had covered.

In the Laboratory setting where library facilities were excellent and where copies of articles were provided on request, few researchers felt the need to subscribe personally to a large number of journals. Therefore the median number of subscriptions per person was lower than in the Minnesota group. Because so much specialized literature was available to them, the Laboratory psychologists tended to subscribe to general publications such as Psychol. Rev., Contemporary Psychol., and Science.

APA-PSIEP Report #12. Theoretical and methodological considerations in undertaking innovations in scientific information exchange. To quote from the Preface of this report, "The accumlated findings of the Project have gradually focused attention upon the possibility of innovation." Studies described in the first eleven reports showed that scientific information flow depends upon a complex system of elements such as journals, conventions, etc., and a corps of active scientists who utilize existing elements and institute new elements to obtain the information they need and to disseminate the results of their own research. The development of new elements appears to be fortuitous: suggestions for innovation are made without consideration of the effect of such a change on the entire "informational economy".

The Project has shown that many channels of information flow exist in the community of American psychologists. A researcher can take advantage of each channel at a definite stage in the process of conducting and reporting research. Some of these channels pass oral communication; others, written. Some are formal; others, informal. Some are activated as soon as a report can be assembled; others, many months later. The Project's findings suggest that innovations involving one channel should not be undertaken without considering the ways in which use of other channels will be affected. If after such consideration the innovation still seems worthwhile, there should be some provision for observing or measuring the effects of the innovation on the system, on the information behavior of scientists, and on the conduct of scientific work. That is, innovation should be undertaken as an experiment and its outcome studied closely.

The first part of this report analyzes the effects of innovation on the entire process of dissemination and considers the probable effect of a number of specific innovations. The second part describes the method and gives control data for examining the effects of a single innovation (the publishing of a Proceedings for the annual APA convention) on the information exchange behavior surrounding a convention presentation and on the subsequent work of the author and of those who learn, by any means, of his work.

In considering the effects of innovation on the process of scientific information exchange it is helpful to visualize the conduct and reporting of a typical research project over a span of five years. Taking as time zero the date of journal publication, the research typically began at -36 months (i.e., three years before). The work reached a reportable stage at about -20 months and was probably circulated in some written form and also presented orally at colloquia and meetings between -20 and -12 months. At about -8 months an article based on the research was submitted to a journal. During this period preprints were distributed and the author may have entered a second phase of oral presentation, perhaps at an invited conference. If the research was supported by an organization desiring a final report, such a document was probably sent off at about this time. After the article is published, reprints are distributed, first perhaps to a mailing list and subsequently as requested. By about +12 months _Pyschological Abstracts_ has abstracted the article. At about +18 months or later (depending on the subject matter reviewed in a given year), the article may be mentioned in the _Annual Review of Psychology_. Roughly five years has then elapsed since the research was begun.

If innovations are introduced into this series at any point, the significance of other events may be greatly altered. As an example, it has been proposed that the core APA journals restrict their contents to longer articles reporting series of studies, reviewing programs of research, etc. Such an innovation, it has been suggested, would reduce the number of submitted papers by discouraging the submission of competent but minor contributions.

Such a proposal has several implications for other elements in the system, however. Since journal publication of any single study would be delayed, the use of journals as literature sources for ongoing research would decrease. The secondary sources (e.g., *Psychological Abstracts*) would also report research later and thus be less useful. Eventually the glut of unpublished material, coupled with researchers' needs to learn of current research more quickly, would lead to the creation of new journals.

A second proposal, that of publishing a *Proceedings of the APA Convention*, promises to have the opposite effect on the twin problems of publishing delay and information overload. It has been suggested that each paper accepted by the divisional program committees of the APA annual convention be published, in an 1800-word version, in such a *Proceedings*. Since papers presented at the national convention account for a significant proportion of the contents of twenty or so primary psychological journals, a *Proceedings* would ease the pressure on these journals to the extent that authors would be satisfied with the *Proceedings* publication and not seek republication in a journal. Moreover, since the *Proceedings* could be made available at the time of the convention, a year or more may be saved in the dissemination of a sufficiently complete report to a national readership.

If it is proposed to have copies of the *Proceedings* available at convention time, it is necessary to consider the ways in which the present information exchange at paper-reading sessions would be altered. In order to determine just what kinds of information exchange are now found in such sessions, a sample of 39 paper sessions and 20 symposia at the 1963 APA convention was studied. Members of the Project staff and other psychologists were trained in the tasks of monitoring these sessions. The primary purpose was to obtain sufficient information to enable the Project to poll samples of attendants after the convention. Monitors recorded the number of persons present at each session, the names and addresses of at least two attendants, and the names and addresses of questioners. In addition, each person making a presentation was asked to record the names and addresses of those who requested copies of his presentation. Participants, attendants, and requestors were polled by mail, and 240, 467, and 202 useable questionnaires were returned by the three groups, respectively.

Five groups of non-participants could be identified from responses. Active attendants (158) discussed a presentation with an author, whereas passive attendants (309) did not. Questioners (68) directed a question to the author from the floor. Attending requestors (52) sought copies of papers they had heard. Non-attending requestors (150) sought copies of papers they had not heard. Both participants and non-participants generally held doctorates, but among the non-participants only questioners had as high a proportion of doctorates as the participants. Participants were generally younger than the non-participants and were involved primarily in research, whereas non-participants had more clinical and applied responsibilities.

As a result of convention feedback about his presentation, one of every five participants who planned subsequent publication of a presentation modified the intended article in some way, usually to make certain portions more detailed or explicit. One of every three modified his ongoing or planned work. Of the non-participants, requestors and active attendants were most active in the area of research covered by the presentation, and they reported the largest percentages of modification in ongoing or planned work. Passive attendants and questioners were generally less active in the area of the presentation; they reported substantially fewer modifications in ongoing or planned work as a result of the presentation.

In general (to quote the somewhat strained prose of the conclusion of the report), "persons having conducted research prior to the convention and being most involved in ongoing and planned research (all such research being in the same area as that of the presentation) made the greatest number of modifications in their ongoing and planned activities as a result of the convention presentation and all other forms of scientific information exchange pertaining to it."

Status of the APA project. A report of the results of the experimental publication of a Proceedings of the APA Convention may be in print by the time this review is read, but the significance of the innovation (and of the way in which the innovation was introduced and studied) cannot be assessed at the time of this writing. Even so, the value of the project as a series of use studies can be assessed on the basis of the first eleven Reports. Certainly this value is very great, perhaps without parallel in all previous research on scientific information flow. A body of comparative data has been established that can be augmented by similar projects in other behavioral sciences.

One serious and pervasive methodological defect must be charged against the project. Poor response was characteristic of the many waves of questionnaire mailings. When the response rate falls below two-thirds (as it did in a few of the substudies), the inference that responding psychologists fairly represent the sampled population of psychologists is probably unjustified. Psychologists who select themselves out of a sample drawn by their own organization, especially when they have been made aware of the objectives of the study, are likely to differ on some attribute from others who comply. The investigators sought to defend against this possibility (*post hoc*) by comparing respondents with the total APA membership on available demographic attributes, but the attributes we would expect to distinguish responders from nonresponders (such as general busyness, attitudes toward the APA, interest in the problem of information flow) have not been examined.

Subsampling nonresponders to determine ways in which they differ from responders is a routine practice in survey research, and it is surprising that the APA staff did not resort to it when response rates fell so low. The possibility of self-selection bias is obvious, and the inference that these information-use patterns are characteristic of American psychologists (for comparision with other groups of scientists) is weaker because of it.

The well-publicized role of the APA in this project may constitute another defect. Several questions dealt with APA-sponsored information services -- the APA journals, Psychological Abstracts, the APA conventions. Evaluation and reported use of these "official" information services may have been biased (either positively or negatively) by the dual role of the APA. An associated problem concerns the psychologist's willingness to report lax information-seeking behavior. Even if he remains personally anonymous in all analyses, he may be expected to realize that his report contributes to totals summed over various aggregates with which he identifies (e.g., the aggregate of all psychologists at his institution, the aggregate of all psychologists working in his specialty). In questionnaires returned to "the home office" it may have been tempting to uphold the industriousness of these aggregates and to recall disproportionate use of the most-respected information sources. Downright falsification is not implied here, since poor recall of information-seeking acts would be a sufficient condition for what might be called error of measurement, and such errors might be expected to favor certain sources and certain activities. Both of these problems suggest the value of a validating substudy conducted by a research team clearly not connected with the APA.

Possible biases in "official" studies of information exchange will recur as a problem in other fields, no doubt. Whether or not a scientist perceives certain activities as more prestigious than others is an empirical question. Whether or not he is more likely to emphasize prestigious information-seeking activities when the inquiry is sponsored by his own professional organization (than when sponsorship is external or simply unspecified) is also an empirical question. The validity of "official" studies depends on a negative answer at least to the second question.

A questionnaire-based study is inevitably stronger in the "what" of behavior at the expense of "why", since useful introspection is difficult to induce and guide without spontaneous probes. Knowledge of the perceived costs and rewards of information-seeking, of perceived functions of information from various sources, of reasons for preferring one source over another, etc., has not been greatly advanced by the APA project (nor was this the project's objective).

IV

THE SYSTEMIC APPROACH:

STUDIES OF COMMUNICATION ARTIFACTS

Most use studies have ignored one variable of great interest in scientific information flow: changing patterns over time. Shaw (1956) came close to creating a time variable by replicating his study one year later, but we expect that more time must intervene for evolutionary changes to be discernible. Shaw hoped to find, and found, consistencies rather than differences. The validity of replicated use studies is challenged, moreover, by maturational factors at work in the particular groups of scientists under study.

In order to detect changes over time, and in order to grasp macroscopic patterns of information flow, a systemic approach is desirable. Where individual scientists are the units of analysis in use studies, the communication artifacts they place in the system are the units of analysis at the systemic level (together, of course, with noncommunication variables, such as number of laboratories, total scientific manpower, national expenditure for research, allocation of funds to specific research activities, etc.).

Some communication events create artifacts; others can only be verified by the recollection of participants. In a study to be reviewed below, Dahling (1962) was able to chart a network of citations showing the rapid application of Shannon's information theory in diverse fields of science, but the network only partly traces (as his title asserts) "the spread of an idea". To suggest an analogy, reported cases of typhoid are a sufficient but not a necessary condition for inferring the presence of the germ. Dahling found no citation evidence that Shannon's information theory had entered such fields as sociology and political science, but undoubtedly some sociologists and some political scientists have long since incorporated it in their thinking about communication behaviors in their fields.

The archival literature has been the source of most communication artifacts studied thus far. Scientific meetings are a second important source of artifacts. Sometimes <u>requests</u> to use the archival literature (e.g., call slips processed at a library) are themselves artifacts inviting study. Studies reviewed in this section draw their data from these sources.

A Systemic View of the Evolution of Little Science into Big Science

Most investigators of scientific information flow are not historians and do not attempt to cast their findings in historical perspective. When an historian works over the same data, what have appeared to be present-day anomalies (e.g., multiple authorship) fall into place on impressively regular curves of growth and development. Derek de Solla Price's Little Science, Big Science (1963) is justly celebrated in the field for its integrative power. This work will not be summarized here (most readers will be familiar with it, and those who have not read it should not be tempted to forego that pleasure), but certain of Price's themes and conclusions may be stated as points of departure for later discussion:

(1) The development of Little Science into Big Science was evolutionary, not revolutionary. All available yardsticks show that science reached its present size through a cycle of doublings that still continue. For instance, the literature of many fields of science doubles every 10 years; the number of scientific journals founded doubles every 15 years; the number of "important discoveries" in science doubles every 20 years (in all these instances, the doubling period is only approximate).

(2) If the number of living scientists doubles every 20 years, then about 80 per cent of all the scientists who ever lived are now alive. If the number doubles every 10 years, then about 95 per cent of all the scientists

who ever lived are now alive. Price suggests 87.5 per cent as an approximate "coefficient of immediacy". Whatever the exact period of doubling, the implication of the fact of doubling is that, in <u>all</u> periods of history since the birth of science, almost all the scientists who ever lived were then alive.

(3) Such exponential growth is not "normal": "In the real world things do not grow and grow until they reach infinity." The full course of development is represented by a logistic curve which increases exponentially only at first, then passes into a nearly linear phase and finally decelerates to an asymptote. It appears that science's rate of growth will eventually cease to be exponential, that finally it will respond to its "saturation limit" either by ceasing to grow, by oscillating erratically, or by "escalating" into a new growth curve determined by changed conditions.

(4) The number of noteworthy scientists doubles more slowly than the total number of scientists. As a consequence, the proportion of noteworthy scientists in the population of scientists declines. There are demonstrable regularities in the production of scientific papers, and " . . . for a field containing 1000 papers, there will be about 300 authors. About 180 of them will not get beyond their first paper, but another 30 will be

above our cutoff [for noteworthy scientists] of 10 papers each, and 10 will be highly prolific, major contributors."

(5) In a normally productive field, a scientist may be able to monitor the output of a colleague group that numbers a few hundred members. Because of limitations inherent in the scientist himself (e.g., reading speed), this number remains relatively constant although the field with which he identifies doubles in size every 15 years or so. "When in the course of natural growth [the field] begins sensibly to exceed the few hundred members postulated, each man will find himself unable to monitor [it] properly."

(6) "[A] noteworthy phenomenon of human engineering is that new groups of scientists emerge, groups composed of our maximal 100 colleagues. In the beginning, when no more than this number existed in a country, they could compose themselves as the Royal Society or the American Philosophical Society. At a later stage, they could split into specialist societies of this size. Now, even the smallest branches of subject matter tend to exceed such membership, and the major groups contain tens and hundreds of thousands. In a group of such size, there are likely to be a few groups of magnitude 100, each containing a set of interacting leaders. We now see

such groups emerging, somewhat bashfully, as separate entities."

(7) Therefore the "invisible college" is not really new: "And so these groups devise mechanisms for day-to-day communication. There is an elaborate apparatus for sending out not merely reprints of publications but preprints and pre-preprints of work in progress and results about to be achieved. . . . In addition to the mailing of preprints, ways and means are being found for physical juxtaposition of the members. . . . For each group there exists a sort of commuting circuit of institutions, research centers, and summer schools giving them an opportunity to meet piecemeal, so that over an interval of a few years everybody who is anybody has worked with everybody else in the same category."

(8) Government support and other factors have led to an era of team research. As an artifact of this, ". . . the proportion of multi-author papers has accelerated steadily and powerfully, and it is now so large that if it continues at the present rate, by 1980 the single-author paper will be extinct." Scientists now communicate person to person instead of paper to paper. Knowledge is diffused through collaboration. Prestige is sought within the select group. "All this . . . has made the scientific paper, in many ways, an art that is dead or dying."

These themes stand in forceful contrast to the limited assertions of Parts II and III of this review. Yet the systemic and behavioral approaches complement each other. Most of Price's propositions (e.g., that a scientist can monitor the output of only x colleagues, that status formerly conferred by publication is now conferred by position within an "invisible college") cannot be tested systemically. Empirical corroboration of these propositions must come from studies of individual scientists.

Systemic Studies of the Literature

A long separate review would be necessary to represent adequately the number and variety of studies that have derived their data from citation behavior, request behavior, and publication statistics *per se*. Only a few studies related to four topics will be reviewed in this section.

What channels are cited. As was true of use studies, most of the cumulative value of these systemic studies is lost because investigators have adopted noncomparable data collection procedures. The following summary of channel citations reported in three studies shows the extent to which idiosyncratic classification schemes limit useful comparison across studies. The data are derived from Patterson (1945), a citation count of the 1939 volume of *Industrial and Engineering Chemistry*; Fussler (1949), counts of randomly sampled sections of the 1939 volumes (among others) of the *Journal of the American Chemical Society* and the *Physical Review*; and Kessler and Heart (1962), a count of the *Physical Review* from 1950 to 1958. The table reports percentages of citations falling into each channel category. A dash indicates that the investigator did not use a category thus defined.

Channel citations by journal and reference year:

Journal: Year(s):	I.&E.C. 1939	J.A.C.S. 1939	P.R. 1939	P.R. 1950-58
Channel:				
Journals and other serials	75.8%	92.7%	91.8%	83.1%
Theses	.8	.6	.4	.2
Patents	11.0	1.5	.07	-
Books	-	-	-	6.7
Books and pamphlets	10.0	-	-	-
Monographs	-	5.2	7.8	-
Personal communication	1.5	-	-	-
Personal communication and unpublished works	-	-	-	8.5
Unpublished papers and addresses	.6	-	-	-
Reports and memoranda	-	-	-	1.4
Base:	4167	1171	1396	137,108

Accepting the relative ranges of these percentages if not the values themselves as representative of science citation behavior in general, it can be seen that references to journals are about ten times as common as references to any other channel. Such acknowledgement of the importance of the serial literature is interesting in itself, even if we do not risk the probably untenable assumption that information inputs via the journal channel were crucial, in that proportion, in shaping the reported research. Books and unpublished papers may each account for one-twelfth or so of all citations; the missing time-series data on these channels would be especially interesting.

<u>Obsolescence rate of journals</u>. Studies of the age distribution of journals currently being cited or requested have shown that obsolescence overtakes the serial literature much more quickly in some fields than in others. Various mathematical functions have been fitted to the empirically exponential curve of declining use of older journals, but perhaps the most useful statistic is simply the median age of all journals currently cited or requested. Burton and Kebler (1960), analyzing their own data on engineering journals and data from six other scientific fields collected by Brown (1956), report median ages as low as 3.9 years (metallurgical engineering) and as high as 11.8 years (geology). Cole (1963), reanalyzing Urquhart's early study (1948), found a median age of 5.9 years in <u>requests</u> for journals undifferentiated by scientific field. Other data reported by Cole, based on small counts of journal <u>usage</u> (not citation), show median ages as low as 1.3 years in petroleum engineering. Urquhart's large-sample study of requests (1959) points to a median journal age of about five years.

Bourne (1963) synthesized the findings of 28 journal obsolescence studies in physics, chemistry, and medicine, showing that there is substantial variation within fields as well as between fields. After dealing with two counterexplanations (variance was not accounted for by size of sample or the particular year studied), Bourne concludes, " . . . the half-life figures now take on a probabilistic rather than a deterministic manner, and we now talk of half-lives in terms of 'variance' and 'best estimates' and 'confidence figures'."

A useful methodological footnote is provided by Bourne's finding that citation counting, request counting, and usage counting methods yield equivalent curves. It had been argued that the methods would yield different curves, since the population of journal authors is quite different from the population of journal users.

Substituting the individual paper as the unit of analysis, Price (1965) showed that "classic" and "ephemeral" papers within a field obsolesce at different rates. Therefore the finding that citations within a field or subfield imply a median age of x years should be qualified by the location in time of certain "classic" papers that are contributing disproportionately to the total sample of citations.

Reference scattering. Just as there is a curve of declining citation and use of older journals, there is a curve of declining citation of minor and peripheral journals in any field, regardless of age. This phenomenon has been described in the literature as "reference scatter", a term apparently introduced by Bradford (1950). An example of scattering is provided by the large sample of Physical Review citations analyzed by Kessler and Heart (1962). Of the 137,108 citations, 68,162 were references to the Physical Review itself. The other 50.3 per cent of all references were distributed as follows:

Number of citations	Number of titles furnishing this many citations
4252	1
3725	1
2000-2999	3
1000-1999	9
100-999	43
50-99	25
25-49	32
10-24	79
5-9	88
<5	519

This distribution leads Kessler and Heart to define "three classes of periodic literature in science": (a) a definitive journal, (b) a closed list of widely used journals, (c) an open list of rarely used journals. It is likely that the difference between the "closed" and "open" lists only reflects the statistical artifact of stable common events and unstable rare events. If a journal accounts for a very small fraction of all citations in a source journal, it may be expected to appear in some samples and disappear in others, while a journal that accounts for a large fraction of all citations may be expected to appear in all samples and thus win a place on the "closed" list.

Cole (1962) brought a degree of order to the literature on reference scattering and suggested that different amounts of scatter are attributable to different organizations of literature within fields. He also provided data showing that, within the same field, an abstract count and a reference-question count yielded nearly identical "coefficients of scatter" (defined as the slope of the essentially linear distribution obtained when cumulative total citations is plotted against the logarithm of cumulative total titles), while a citation count from a source journal yielded discrepant results (much less scatter). This is reasonable, since the first two methods have no "anchor" in the serial literature, while the third method picks up only those journals that may logically be cited in the source journal.

Bourne (1963) plotted scatter distributions for data obtained from 27 studies and demonstrated great differences in the number of journals required to account for 50 per cent (or 90 per cent, or whatever fraction) of all citations in a sample. In two studies, just one journal provided more than 50 per cent of all citations. Fewer than 10 journals provided 50 per cent of all citations in 8 other studies. Between 11 and 100 journals were needed to account for 50 per cent of all citations in 12 other studies, and in 3 studies more than 100 journals were required.

Such differences in scatter have great significance in information flow, and research is needed to determine what underlying factors account for the variation. Certainly narrow specialties and broad areas will differ in the scatter of their serial literatures, and the existence of a "definitive journal" may be a function jointly of the age of the discipline and of the dominance of one professional organization within it. Scatter deserves to be quantified as precisely as possible within each specialty, since it is one of the few environmental constraints that handicaps researchers similarly within specialties and differently between specialties.

Bibliographic coupling: (1) paper to paper. Bibliographic coupling between papers has been defined by Kessler (1963) as the sharing of an item of reference by two papers. This is acceptable as a general definition of the phenomenon if it is understood that each paper implicitly cites itself, so that the first paper in a sequence is not lost from the network simply because it does not share citations in its reference list with later papers.

This systemic approach to the study of information flow is quite new. Dahling's study (1962), completed in the late 1950's, is the earliest the reviewer could find. Dahling seems to have borrowed his orientation and terminology from the literature on the diffusion of innovations; his dependent variable is diffusion rather than coupling, although the data can be analyzed either way.

Dahling's topic and methodology have much to commend them to future historians of science. He was interested in the rapid adoption of Shannon's information theory in fields other than telecommunication, and he studied its diffusion by means of citations. His task was simplified by a known, recent "point zero" -- the publication of Shannon's initial article on the subject in 1948 (sharing at least part of the priority was Wiener's work on cybernetics, also published in 1948). Dahling found via citations that information theory was introduced into psychology in 1949, into physics, biology, physiology, sociology, and linguistics in 1950, into statistics in 1951, etc. In some of these fields the theory was being applied only metaphorically, to be sure, but the nature of the application is irrelevant to the demonstrated fact of interdisciplinary coupling. By 1955, the citation evidence indicates, the theory had made its impact on at least 17 fields. Dahling plotted the network of linkages within and between fields, showing that some papers became sociometric "stars" while others became "isolates".

Independently, apparently without knowledge of Dahling's work, and with quite different objectives, Kessler (1962) introduced the notion of bibliographic coupling to the literature of information storage and retrieval. He suggested two criteria of paper interrelatedness based on the measure of shared reference: (1) "A number of papers constitute a related group G_A if each member of the group has at least one coupling unit to a given test paper." (2) "A number of papers constitute a related group G_B if each member of the group has at least one coupling unit to every other member of the group." (Note in passing that Dahling's study established the existence of a G_A group: every paper in the sequence cited either Shannon's or Wiener's 1948 publications, which may be construed jointly as the test paper.) The degree of relatedness within either group is a function of the number of couplings with the test paper or with every other paper in the group.

From his nine-year sample of Physical Review papers Kessler chose ten test papers for case studies of groups of related papers constituted empirically via bibliographic coupling. One of the test papers was serially first in the sample; it offered a maximum coupling span into the future. Another paper was serially last and offered a maximum coupling span into the past. A test paper was chosen to represent a very active and popular field of research, while another represented an inactive classical topic in physics.

A paper chosen from the middle of the time sample typically yielded a bell-shaped (but not necessarily normal) distribution of couplings with earlier and later papers. That is, the probability that it shared references with earlier papers decreased with age, while the probability that later papers shared references with it similarly decreased. The first and last papers in the sample showed one-sided distributions, of course, and the longer tails of these distributions indicated an asymptote of essentially zero couplings per volume after nine years.

The paper chosen to represent an active research area coupled with 322 other Physical Review papers in this nine-year sample. Since 322 papers do not constitute a useful group for search purposes, Kessler raised the criterion of relatedness to four or more couplings and found an inner group of 18 strongly related papers.

In the case of the paper chosen to represent an inactive classical area of research, only five coupling papers were found in the nine years. Therefore Kessler expanded this G_A group by construing each of the five papers as a test paper to find additional couplings. Twenty additional papers coupled to the five, creating a multi-nodal network that would be interesting to study with all linkages drawn.

Kessler's interest in coupling concerns its value as a bibliographic tool in searching a literature for retrieval of relevant papers. Investigators of information flow could borrow Kessler's method and terminology to study networks of papers "sociometrically". It would be necessary, of course, to follow the network into other journals, and perhaps into books and informal communication channels as well.

Garfield, Sher, and Torpie (1964) used direct citation patterns rather than shared references to trace the network of papers which led (together, of course, with other information media) to the discovery of the DNA code. Asimov's historical review of DNA research, The Genetic Code, provided entries into the network. This work specified certain connections among research projects and implied other connections. The pattern of these connections was compared with the pattern of linkages established via citation analysis (data were drawn from the Garfield and Sher Genetics Citation Index). Good but not perfect agreement was found between the two networks. There are three problems that will weigh in future decisions to use citation indexes as historiographic tools: (1) When two papers are being published within a few months of each other, it is relatively difficult for either to cite the other, and therefore this linkage is lost in a direct citation analysis (of course, if the papers belong to a group, the linkage will be detected via bibliographic coupling). (2) If a researcher happens to be unaware of related work elsewhere, he will fail to make the citation that would create a linkage; the historian may in retrospect identify independent research projects as related. (3) In a rapidly advancing field, particularly, informal information exchange may be more decisive than published papers in coordinating effort and apprising researchers of recent developments.

Price (1965) used bibliographic coupling to shed light on "the total world network of scientific papers". He showed first that the distribution of citations per paper is bimodal: about 9.5 per cent of the papers listed no references at all, while there were about 5 per cent each of papers listing 3 to 10 references. Thereafter the curve falls off quickly; only slightly more than 1 per cent of the papers, for instance, list 25 references.

Turning his attention to cited papers rather than the citing paper, Price found that about 35 per cent of existing papers seemed not to have been cited at all in a given year, while another 49 per cent were cited only once. About 9 per cent are cited twice; 3 per cent, three times; 2 per cent, four times; 1 per cent, five times, and a remaining 1 per cent, six times or more. He concluded that some 4 per cent of all papers appear to be "classics", cited four or more times in a year.

He infers that the process reaches a steady state: about 10 per cent of the papers will not have been cited at all, another 10 per cent cited once, 9 per cent twice, etc., while about half of all papers will have been cited five times or more.

Price analyzed coupling within a network of 200 papers that constitute the entire literature on a peculiarly isolated topic (the spurious phenomenon of N-rays, ca. 1904). A diagonal matrix was used to represent the coupling pattern (e.g., a mark in row 11 of column 87 indicates that paper 87 cited paper 11). There are three interesting regularities within the matrix: (1) the bulk of all citation involves the 30 or 40 papers immediately preceding the citing paper, (2) review papers with long reference lists stand out as strong columns, (3) "classic" much-cited papers stand out as strong rows. Review papers recur with remarkable periodicity; after every 40 research papers, another review paper seems to be needed. The "classic" papers occur after various time lapses; apparently the cumulation of research papers does not generate a "classic" paper as inevitably as it generates a review paper. The review papers, incidentally, were not greatly cited by subsequent research papers.

Bibliographic coupling: (2) journal to journal. With different objectives, the coupling of journals has also been studied. The pattern of citation from journal to journal can be interpreted as delimiting a field, just as the pattern of citation from paper to paper delimits a single research topic. Xhignesse and Osgood (1963) contributed a study of journal coupling to the APA Project on Scientific Information Exchange in Psychology. Relationships among 21 psychology journals were studied; the reader may regret with the authors that the sample of journals was not larger.

Reciprocal citation, rare among papers, is very common among journals. The investigator can take advantage of reciprocal citation to measure symmetry within the network (cf. the Xhignesse-Osgood measure of congruence, the correlation between a journal's citation of each of the other journals in the network and the citation by each of the other journals of that journal). These measures impose an unfortunate constraint on the study design: the network must be closed; each cited journal must also be included as a citing journal, with only a residual category provided for citations outside of the network.

When the investigator can logically construe the network as closed, it is probably worth doing so in order to make use of reciprocal measures. In addition to the measure of congruence, Xhignesse and Osgood demonstrate the value of the "filter/condenser ratio" (relative evenness with which a journal is cited by other journals in the network divided by the relative evenness with which it cites other journals itself), the "index of balance", the "self-feeding" measure, etc.

Some studies of journal-to-journal coupling must focus on open networks. If, for instance, the journals of an emerging interdisciplinary research area are being studied, it must be recognized that each journal has strong and interesting couplings with journals <u>within</u> traditional disciplines. It is impossible, given present computer capacities and the expense of preparing a citation file for the computer, to enlarge the group of <u>citing</u> journals to encompass all those that are cited by the original group. Therefore the citation matrix cannot be square; there will be a relatively small group of citing journals (the logically constituted interdisciplinary network) and a large group of cited journals.

By collecting data on the same set of journals in 1950 and in 1960, Xhignesse and Osgood were able to detect changes in the organization of their closed network over 10 years. In general the changes were very slight; the stable network implies a relatively mature discipline. Within each of the two time samples the clustering of journals (subnetworks within the network) was studied via reciprocal citation. Not surprisingly, a rather tight cluster of experimental, educational, social, and general psychology journals was obtained, with psychiatric and psychoanalytic journals conspicuous as "isolates". These latter journals, interestingly enough, did not form a citing c[illegible] their own; this finding may reflect the competitiveness o[illegible]chiatric "schools".

Systemic Studies of Scientific Meetings

Several of the APA-PSIEP Reports (#s 3, 4, 5, 6, 8, 10), reviewed for the sake of continuity in Part III, deal systemically with the role of scientific meetings in information flow. The third report covers a 25-year time sample of national APA conventions. Other reports chart the publication fate of convention publications, compare the functions of regional, national, and select meetings, etc. The reader may wish to look at those reports again, in conjunction with studies to be reviewed in this section.

Growth rate in biomedical meetings and travel. As discussed above, Price (1963) presents a strong case for stable "doubling periods" associated with all aspects of the transition from Little Science to Big Science. He also argues convincingly that observed exponential growth is characteristic only of early phases in the full logistic curves of development; as growth approaches saturation (to pose an extreme example: as expenditure on scientific research approaches the gross national product), the curve either decelerates smoothly to an asymptote, oscillates wildly, or escalates into a new early curve as a result of changed conditions. A study of the growth rate in biomedical meetings and travel by Orr, Coyl, and Leeds (1964) suggests that these communication phenomena are in the early, exponential phase of their full development.

Although the war years depressed growth, the number of regular biomedical meetings in the U.S. appears to double every 20 years or so -- somewhat slower, that is, than scientific journals, which double in number every 15 years according to Price's estimate. The doubling period of the number of biomedical societies also seems to be about 20 years, but growth during the 1940's was well below the predicted level. The second finding disturbs the authors more than the first, since the "proliferation of societies" (their term) has apparently outstripped the ability of the information system to announce forthcoming meetings: "only one out of four regular meetings of U.S. biomedical societies are announced by these major services collectively."

The number of papers presented at meetings of the Federation of American Societies for Experimental Biology shows a linear increase over 20 years, while attendance has grown exponentially (neither curve is smooth enough, however, for a confident statement of the underlying function). The fact that meeting attendance more than doubled in the past decade while the number of meetings increased more slowly may explain in part why the number of meetings and the number of societies can have a longer doubling period than journals: the capacity of a journal to grow is quite limited, but societies and the meetings they sponsor can absorb great increases in members and papers.

Data on the increase in travel are more skimpy. The number of biomedical personnel working abroad was about 50 per cent higher in 1962 than in 1952, but increases of 140 per cent and higher are cited for personnel in other fields of science. The proportion of NIH research grant funds allocated to travel has increased from 1.67

per cent in 1947 to 2.53 per cent in 1960, and the total dollar value of travel expenditures from this source has grown exponentially.

<u>The publication fate of convention presentations</u>. One of the findings of the APA-PSIEP investigation was that fewer than 50 per cent of papers and symposia presented at the 1957 APA convention were eventually published in an archival journal. Liebesny (1959) studied the publication fate of papers presented at the 1948 and 1949 Annual Meetings of the Optical Society of America, the 1949 National Convention of the Institute of Radio Engineers, and the 1950 Meeting of the American Physical Society; on the average, 51.5 per cent of these papers received publication. There was no systematic difference in publication rate by field.

Leibesny distinguished invited from uninvited papers and computed publication rates separately. Slightly <u>fewer</u> invited papers were published. He concluded: "Thus nearly half of the information presented at such meetings appears to be lost, unless some preprints are available, or unless the author is approached directly for copies of the manuscript."

V

THE FLOW OF (BEHAVIORAL) SCIENCE INFORMATION TO THE PUBLIC

Receivers of scientific information can be arrayed along a continuum of previous knowledge of the research area being described. At one end of the continuum are a scientist's colleagues within his specialty. This small group of insiders knows the background of his research; they need to be informed only of latest findings. Next in line are other scientists in his discipline; they may need to be informed of background as well as findings, but they probably understand his terminology. Farther along the continuum are scientists in other disciplines; they need to be informed of background, findings, and terminology, but they understand general procedures, the criteria of reliability and validity, etc.

Still farther along the continuum, reaching to the far end, are members of the general public. Depending on educational background and interest in science, the public may need complete information on terminology, background, findings, and even (or perhaps especially) the ground rules of scientific investigation.

If data were available, it would be pertinent to review what has been learned of the flow of information from the (behavioral) scientist to receivers located at various points along this continuum. In the absence of data dealing with intermediate points along the continuum, some studies focusing on the flow of information to the general public will be reviewed.

Reviews of public knowledge of science and attitudes toward science. Schramm (1962) has reviewed much of this literature, and some studies not mentioned here, in a memorandum for the American Association for the Advancement of Science. He stresses the consequences of science information flow -- public knowledge of science and attitudes toward science -- rather than the flow itself. Data were found to support twelve propositions concerning knowledge and attitudes, of which six are most germane to this review:

(1) Knowledge of science is widely, but not deeply, distributed in the United States. There are still large areas of ignorance.

(2) An individual's education is the chief predictor of his science knowledge.

(3) Mass media use is the second predictor of scientific information; after the school years, most of the increment of science knowledge comes from the media.

(4) Where one goes for scientific information depends on the topic and one's own characteristics.

(5) Public attitudes toward science and scientists are generally favorable, although not very accurately informed.

(6) The public is interested in getting more scientific information.

Tichenor (1965) performed secondary analyses of a variety of national surveys, most of which contained only a few relevant questions, to show how knowledge of science and attitudes toward science are distributed throughout the adult population of the United States. Like Schramm, he was not interested in information-flow variables _per se_ but in their consequences.

Variables relevant to _this_ review concern _what scientific information_ reaches _which members of the public_ through _which channels of communication_. No effort will be made to represent systematically the content on public knowledge of science and public attitudes toward science. The reader interested in these effects should consult the reviews by Schramm and Tichenor.

Public Exposure to Science Information

Two national-sample surveys, conducted by the Survey Research Center in 1957 and 1958, have provided summary data on public exposure to science information. Both surveys were sponsored by the National Association of Science Writers and New York University, with support from the Rockefeller Foundation. Most data pertinent to this review were collected in the first survey; the second survey was a post-Sputnik sequel to detect changes in science information intake in the months following that highly publicized launching. There were 1919 respondents in the first survey, 1547 in the second.

Recall of science news in the media. Taking recall as a conservative measure of exposure to nonmedical science information, SRC found that 52 per cent of the 1957 sample could recall at least one recent science item in the news. When medical science news is added, the percentage of respondents recalling one or the other types of science news is 75.6. Since not all medical news in the media qualifies as "science information", only nonmedical science information exposure will be considered hereafter.

Tabulating on sample attributes, it is found that men recalled a science item significantly more often than women (61 to 45 per cent). Older, less-educated, lower-income respondents were less able to recall a science item. There was no clear trend on such variables as region of the country (except higher recall in the Far West), religion, or urban-rural residence (except higher recall in metropolitan suburbs). Both exceptions indicate a need for analyses controlling on education and income.

Defining those who regularly use each medium as its audience, 40 per cent of the newspaper audience recalled one or more science items in the newspaper, 31 per cent of the magazine audience recalled science items in magazines, and 24 per cent of the television audience recalled science items on television. Only 10 per cent of the radio audience recalled science items on radio.

Across all media, topics of recalled science information were essentially limited to three fields of science. "Technology" items (in the sense of "better things for better living") were recalled more often than others (from 25 per cent in newspapers to 7 per cent on television). Atomic energy items were second in frequency of mention (from 22 per cent in newspapers to 4 per cent on radio), and aeronautics items were third (from 14 per cent in newspapers to 3 per cent on radio). All other sciences and research areas together accounted for only 10 per cent of recalled items. Of all topics from the behavioral sciences, only mental illness was mentioned with any frequency (from 5 per cent in newspapers to less than 1 per cent on radio).

Taking each medium separately, the question of <u>which</u> members of the population recall science items receives a consistent answer:

<u>(1) Recall of science information in newspapers</u>.

- **Sex.** Twenty per cent more men than women recall science items in newspaper (56 versus 36 per cent).
- **Education.** Higher education means higher recall, from 23 per cent among those with grade school education to 73 per cent of those with college education.
- **Income.** Higher income means higher recall, from 15 per cent of those with incomes under $1000 (in 1957) to 62 per cent of those with incomes over $7499.
- **Age.** The 25-29, 35-39, and 40-44 age groups have higher than 50 per cent recall, the other age groups have lower than 50 per cent recall. Only one-third of those in the over-64 age group could recall a science item in the newspaper.
- **Region.** Westerners have higher than 50 per cent recall, other regions less than 50 per cent, but the Northeast -- not the South -- has lowest recall.

- Rural-urban. Metropolitan suburbs and small towns have high recall. Other areas, from central cities to open country, are uniformly about 10 per cent lower.
- Religion. No differences in recall among the three groups.

(2) Recall of science information in magazines.

- Sex. More men than women recall science items in magazines (50 versus 28 per cent).
- Education. Only 25 per cent of grade-school-educated respondents, against 48 per cent of college-educated respondents, recall science items in magazines.
- Income. Only 23 per cent of the lowest income group, against 49 per cent of the highest income group, recall science items in magazines.

There is a slight age trend, with higher recall among younger respondents. The regional trend observed for newspapers continues, with highest recall in the West and lowest recall in the Northeast. Metropolitan suburbs exhibit highest recall of science items in magazines, followed by rural areas -- not small towns. There is no significant difference in recall by religious grouping.

(3) Recall of science information on television. The gap between male recall and female recall narrows in the television audience (38 versus 29 per cent). The same narrowing occurs between education groupings (21 and 47 per cent recall at the extremes) and between income groupings (27 and 42 per cent recall at the extremes).

Most of the narrowing occurs at the expense of the college-educated, higher-income groups who recalled much more science content in newspapers and somewhat more in magazines. Younger respondents recall more television science content than older respondents, but there are only weak regional differences, no significant religious difference, and only a slight tendency for higher recall in small towns and rural areas.

(4) Recall of science information on radio. The sex difference in recall of radio science content is negligible (16 per cent of the men, 14 per cent of the women). Only education, among the seven demographic attributes, is clearly -- although weakly -- associated with recall (11 and 21 per cent recall at the extremes).

Primary sources of science information. Respondents named the media as primary sources of science information in almost the same order as they were able to recall science items in the media.

	Per Cent Recalling One or More Science Item in the Medium	Per Cent Naming the Medium as Primary Source
Newspapers	40	34
Magazines	31	21
Television	24	22
Radio	10	3

The two columns of percentages cannot be compared directly, since the first is based on the audiences of the four media and can sum to more than 100 per cent while the second is based on the entire sample and sums to 100 per cent minus those who could not name a primary source. Yet, except for a reversal of 1 per cent between magazines and television, the rank correlation is perfect.

Respondents were also asked to make a secondary choice. Among those who named newspapers as primary choice, television was the modal secondary choice. This was a reciprocal relationship; intitial television choosers named newspapers second. Otherwise, those who named a print or broadcast medium as primary source remained faithful to that communication mode: magazine choosers named newspapers second, and radio choosers named television second.

There were certain demographic patterns in primary source preference:

- **Sex.** Men more often named magazines as primary source, while women more often named radio and television.
- **Age.** The youngest and oldest respondents were more likely to name broadcast media as primary sources, while those in the 25-64 age group mentioned print media relatively more often.
- **Region.** There were no significant regional differences in choice of primary sources.
- **Rural-urban.** Rural residents were much less likely than residents of other areas to name newspapers as primary source, but they were second only to metropolitan suburbs in naming magazines.

- Religion. Jews were markedly higher than the other religious groups in their preference for magazines and markedly lower in their preference for television.
- Education. Higher education was strongly associated with high preference for magazines and low preference for television. The naming of newspaper and radio as primary sources declined somewhat as education increased.
- Income. Lower-income groups named radio as a primary source much more often than middle- and higher-income groups. Preference for television was relatively constant except for a sharp falling-off in the highest-income group. Newspapers were most often named by the middle-income group, magazines by the higher-income group.

Science "knowledge" and use of the media. A four-item Guttman scale of science information provided at least a crude index of each respondent's science awareness or, narrowly construed, science knowledge. The four items concerned polio vaccine, fluoridation, radioactive fall-out, and space satellites. Respondents were credited with some knowledge of each of the four topics if they could describe the phenomenon, state the purpose of the program, etc. Only 16.9 per cent of the sample responded to all four items correctly while 23.8 per cent answered only one and 8.7 per cent answered none.

As might be expected, both education and income were strongly correlated with the Science Information Scale (SIS) score. Men had slightly higher SIS scores than women. There were other demographic trends (higher SIS in the West, in metropolitan suburbs, among Jews), but these are likely to be artifacts of education-income differences by region, place of residence, and religion.

High SIS score was associated with high use of newspapers and magazines. High SIS respondents were more likely than low SIS respondents to use the radio very little (less than an hour a day), but there was no difference in proportions of high and low SIS respondents using the radio a moderate amount (from one to four hours a day). There was no SIS trend among respondents who use television very little (less than one hour a day), but moderate viewers (one to four hours) have higher SIS scores than heavy viewers.

Preference for newspapers and magazines as primary sources of science information is associated with high SIS score. The trend for magazines is especially strong, from 4 per cent to 44 per cent at the SIS extremes. Preference for television and radio as primary sources for science information is strongest in the intermediate SIS levels, since respondents with SIS scores of 0 and 1 frequently said that they obtained no science information whatever from the media, therefore did not name a primary source.

<u>A motivational typology of science information consumers</u>. "Enthusiastic", "active", "occasional", and "uninterested" consumers of science information were identified on the basis of how much science information they presently obtain from newspapers and how much more science information they would like to see published.

Enthusiasts, for instance, read all or some of the science information now in their newspapers and would like to see more published. Activists were defined as those who read all or some and are content with that amount.

Not surprisingly a strong correlation is found between SIS score and the four motivational types. Highly motivated respondents were also able to recall more science content from each of the media. All four motivational types named the newspaper as major primary source of science information, and equal proportions in the four groups named it. Highly motivated respondents disproportionately named magazines as primary sources; respondents with low motivation to consume science information disproportionately named television.

Enthusiastic consumers of science information tend to be men rather than women. They belong to no particular age group, region, residential area, or religion. Consistent with the recurrent pattern, they have more education and higher incomes.

Attitudinal correlates of science information consumption. Various questions in the 1957 survey invited respondents to express their viewpoints on the role of science in human progress. Although control on education is sorely missed in this analysis, the following relationships between attitude and consumption of science information were found:

(1) The "threat of science". An index of high and low perceived "threat of science" was constructed from responses concerning the negative consequences of science. The threat index was strongly

correlated with respondent's education. Those who perceive high threat were more likely to skip over science items in the newspaper. They were also less likely to recall a science item in any of the media. Whether such differences would survive a control on education was not determined.

(2) Science and a better life. Few respondents were willing to answer negatively such questions as "Would you say that the world is better off or worse off because of science?" Those who so responded were more likely than positive responders to skip over, or merely glance at, science content in newspapers.

(3) The morality of science. About a quarter of the sample expressed doubts about the morality of science in response to questions about the tendency of scientists to "pry", about the prospect that science will permit control by a few, and about the tendency of science to break down morality. The doubters were less likely than the non-doubters to read science items in newspapers.

(4) The understandability of nature and life. Those who agree that events have causes (i.e., are not mere accidents) were somewhat more likely to read science items in newspapers than other respondents who disagree or don't know. Those who believe in a God-governed world did not differ in readership of science from those who believe in a self-governed world. Newspaper science readers were more likely to see the world as understandable and orderly than were those who skipped over, or merely glanced at, newspaper science content.

Composite profile of the science information consumer in the 1957 survey. The Survey Research Center concluded from these analyses that the consumer of science information was likely to be:

- Male,
- Young or middle-aged.
- Well-educated.
- In the higher income brackets.
- A Westerner or Midwesterner.
- A student of science in high school or college.
- An urban dweller but not a resident of the metropolitan central city; he is found in large and medium-sized cities and in the suburbs.
- A heavy user of the media who prefers to receive science and general news via the print media though favoring television for entertainment.
- Favorably disposed towards the goals of scientific inquiry and convinced that the world is orderly and knowable.

Such a set of attributes permits the science information consumer to be located in the general population. In the absence of controlled analyses, however, it cannot be said which of these attributes are functionally related to science information exposure and which are merely artifactual.

Differences in science information exposure from 1957 to 1958. The launching of Sputnik I in the fall of 1957 was a science news event of unique interest and significance. Accordingly, the Survey Research Center replicated its national survey to establish differences in science information intake, in attitudes toward science and scientists, in specific knowledge of the satellite programs, etc.

Although the change cannot conclusively be attributed to Sputnik and its aftermath, fewer respondents named the print media as primary sources of science information in the 1958 survey. In 1957 the print media were named as primary sources by 66 per cent of the sample; in 1958 the percentage was down to 57 while the broadcast media gained correspondingly. Newspapers lost twice as many adherents as did magazines, suggesting that the faster broadcast media were able to capture some of the audience for bulletin news but less of the audience for background news.

While readership of other newspaper content changed little, 9 per cent more respondents read all or some science news in 1958 than in 1957. The content category registering the next largest gain in readership was "people in the news", a change perhaps reflecting greater coverage given to individuals involved in the space program. There is a moderate positive correlation, when non-newspaper-readers are omitted from the analysis, between reading science news and reading about "people in the news".

The greatest increases in exposure to science information occurred among women and the less educated -- the groups least exposed to science information in the 1957 survey. There was actually a slight decline in science information exposure among college-educated men. Regression toward the over-all mean is an alternative explanation of both trends, however.

Other Research on Public Exposure to Science Information

Two important sources of science information -- books and other people -- were not investigated in the 1957 and 1958 Survey Research Center surveys. A very small amount of data on use of these sources has been collected in other studies.

Books. The Public Library Inquiry (University of Michigan, Survey Research Center, 1947 -- reported by Berelson, 1949, and Campbell and Metzner, 1950) established that books on science comprise about 5 per cent of total library circulation to adults. This proportion, which amounts to about 15 per cent of all adult nonfiction circulation, is very stable in libraries serving small, medium, and large populations. The PLI did not establish which users of the public library borrow books on science, but it is reasonable to infer that education and income correlate with science circulation at least to the extent that they correlate with adult nonfiction circulation in general. Parker and Paisley (1965) found in a study of circulation data from 2700 communities that education and income correlate most strongly and positively with adult nonfiction circulation, seven other community characteristics held constant.

Compiling data from studies as early as 1927, Berelson showed that about half of all reference questions submitted to public libraries by adults concerned either social science or natural science. This does not mean, of course, that half of the libraries' reference function is that of answering science questions; patrons are likely to research the simpler (perhaps nonscience) questions themselves and submit only the more difficult (perhaps science) questions to librarians for assistance.

In these studies we have no clear indication of how many people are obtaining science information from library books and library reference services. In surveys conducted in two California cities (Parker, research for the U.S. Office of Education, in progress), it was found that the percentage of respondents reading science books (obtained from whatever source) was very low. Although 47 per cent of respondents in San Mateo and 36 per cent of respondents in Fresno had read at least one book in the month preceding the survey, for a combined average of 2.5 books per book-reading respondent, fewer than 1 per cent of the respondents in either city had read a science book during that period.

Other people. Respondents in the SRC survey for the Public Library Inquiry were asked where they would go for information on four subjects, two of which (nutrition and child-rearing) could be viewed as applied science. The following answers were most frequently obtained:

Nutrition	Child-rearing	
56%	31%	would consult a professional in the field
18%	21%	would consult a book
9%	15%	would consult a family member or friend
8%	4%	would consult a magazine
1%	3%	would use the public library
-	20%	would rely on their own experience

The two interpersonal sources together account for 65 per cent of the choices for information on nutrition and 58 per cent of the choices for information on child-rearing when those who would rely on their own experience are omitted. Thus, whether or not respondents act upon their preferences, it is clear that other people are widely regarded as preferred sources of science information; respondents would consult other people more than half the time when information is needed on such topics as nutrition and child-rearing.

In actuality, the most-mentioned source, a professional in the field, ranks far down on the list of sources respondents have consulted, according to responses to a similar set of questions asked in a San Francisco study (Stanford University, Institute for Communication Research, 1957). Respondents were asked where they would go, and where they had gone, for information on cancer, child-rearing, and mental health. Professional experts were the sources that 93, 73, and 94 per cent of the respondents would consult for information on the three topics, but only 10, 10, and 12 per cent of the respondents had consulted a professional expert. Libraries would be consulted by 31, 50, and 19 per cent, but fewer than 1 per cent had used the library for information on these topics. On the other side, many more respondents had consulted friends and the mass media than would consult them. Therefore, perhaps because preferred sources are less accessible, there is an unreconciled discrepancy between the public's preference for science information sources and the public's actual use of science information sources.

These data cannot provide an estimate of over-all use of other people as science information sources, since responses had reference to specific topics such as child-rearing. In the San Mateo and Fresno surveys respondents were asked what expert sources they had consulted for information of any kind during the month preceding the survey. Omitting health experts (e.g., physicians) and all technicians (e.g., radio repairmen), only 1.7 per cent of the San Mateo respondents and 1.3 per cent of the Fresno respondents named a "scientific expert".

In summary, although the public obtains some science information in the course of regular use of the mass media, such specific acts as reading a science book and consulting a science expert appear to be of low incidence. The data on such information-seeking are particularly inadequate at this time.

How Much Science Information Flows and How It is Presented

It is impossible to compute what fraction of all science information is available to the public through all communication channels. Thistle (1958) estimated that only one hundredth of 1 per cent of all science information can be communicated to the public. This estimate seems too low when each major and much-publicized science is considered separately, but it seems reasonable or perhaps too high against the fact that the National Register of Scientific and Technical Personnel lists about 1150 research specialties, some of which are individually quite broad (e.g., metals and alloys, explosives and rocket fuels).

The corpus of all science information is itself undefined. Does it include every unsuperseded finding from the very beginning of scientific inquiry? Or only the work of recent years? Or, consistent with the concept of "news", only the work that has just now been made public?

The third is the most manageable definition, especially if the focus is narrowed to a single science news-making event. Wood (1962) studied press coverage of the 1955 American Psychological Association convention in San Francisco to determine how much of the research reported at that meeting entered channels of communication to the public. To the reviewer's knowledge, this is the only study that expresses science information communicated to the public as a fraction (albeit imprecise) of the total science information generated by an event.

Of the 587 research reports, symposia, speeches, films and other events at the APA convention, Wood found that 47 were reported in local newspapers, in Time and the New York Times, or in other papers served by the Associated Press. Thus the fraction of coverage was 8.0 per cent. Speeches, symposia, and research reports had coverage fractions of 21.7, 15.6, and 7.6 per cent respectively. None of the other events were reported at all. Articles written about speeches averaged 10.3 column inches; those written about symposia and research reports averaged 8.3 and 5.5 column inches. Even estimating 30 words per column inch (probably too high for this material), these space allotments indicate that about 310 words were written in coverage of a speech and 250 and 160 words, respectively, in coverage of a symposium and a research report. The original text of these presentations must have been at least 10 times as long as the article covering it (in the case of a speech) and perhaps 40 to 50 times as long (in the case of a symposium). Covered events, then, were greatly condensed in reports the public sees.

Allowing for coverage that may have escaped Wood's attention, perhaps it is not wide of the mark to say that fewer than 10 per cent of the events were covered and that less than 10 per cent of the content of each covered event was available even to the local public. If a member of the local public scanned four local papers, Time, and the New York Times for news of the APA convention, he would learn much less than 1 per cent of what had been presented there, especially since two-thirds of the coverage was duplicative.

Small as was the public's share of information from this event, Wood found that reporters and science writers were distorting what the scientists had said. Psychologists reading coverage of their own presentations felt that the writer had missed the point more than 15 per cent of the time. For instance, a study of delinquents' evaluations of the relative seriousness of crimes showed that delinquents rank crimes in the same order as do control groups of nondelinquents. This finding was reported in one story with the lead, "In another of this morning's lectures . . . it turned up that 'There is honor among thieves'." Another story covered the same study under the head, "Delinquents Defended at Science Meet".

More than half the psychologists who commented on coverage of the convention found small errors in the articles. Also noticeable in the coverage was an undercurrent of hostility toward psychologists (e.g., "The answer would appear to be obvious, but psychologists are not happy until they can demonstrate the obvious by measuring it.").

The findings of Wood's study are not encouraging. Much less than 1 per cent of information emanating from the APA convention entered channels of communication to the public. The usefulness of even this light coverage is questionable, since scientists who originated the information found many instances of error and misinterpretation in it.

The proposition that very little science information flows to the public is supported by studies estimating the fraction of all news space allotted to science news. Ubell (1957) reported studies of 29 newspapers in 1938 and 130 newspapers in the period from 1939 to 1950.

About 1 per cent of the nonadvertising space was devoted to science news in each of the samples. Cutlip (1954) monitored the Associated Press trunk line into Wisconsin for sampled weeks from 1950 to 1953 and found an average of less than 1.5 per cent of the news concerned "science and inventions". Less than half of even this small total was picked up from the trunk line for the state wire.

Smythe (1952) and Smythe and Campbell (1951) report that science programming or news on New York and Los Angeles television was about 0.25 per cent of program time; this fraction was quite stable between cities and in two time periods. Science programming is proportionately much greater on educational television, and Schramm, Lyle, and Pool (1963) found that several science programs were regularly followed by more than 10 per cent of the educational television audience, but this audience is so much smaller than the commercial television audience that the impact of science on educational television might best be compared with the impact of science books and science courses in adult education.

Taylor (1957) looked for material relevant to mental health in newspapers, magazines, radio, and television. Although his samples of content were not random (but large, and reasonably representative), Taylor's estimates of mental health content are consistent with other estimates of total science content. He found that 99.9, 98.2, 96.2, and 94.5 per cent of the content of the four media, respectively, contained <u>no</u> material relevant to mental health. Most of the relevant material found in magazines, radio, and television occurred in the context of entertainment (e.g., magazine fiction, soap opera).

Taylor analyzed dominant themes in mental health content in the four media. Psychologists who reviewed his thematic analyses concluded that "the mass media were telling the public the wrong things" about the diagnosis and treatment of mental illness.

The special problems of communicating behavioral science information to the public come to focus in Taylor's study and in the larger project (directed successively by Schramm and by Osgood), on "the communication of mental health information", of which it was a part. To a greater extent than findings in the physical sciences, findings in the behavioral sciences are vulnerable to three great communication impediments: suppression at the source, distortion in midchannel, and censorship at the destination.

In one of the mental health project substudies, Tannenbaum and Gerbner (1962) reviewed the crazy-quilt of restrictions that producers of motion pictures and television programs are expected to observe. The long list of taboos is an unmistakable effort at source suppression.

Distortion in midchannel is evident in the studies by Wood and by Taylor. An ill-concealed hostility toward behavioral scientists shows through popular treatments of their work. Simple misunderstanding distorts some of the objective writing that gets through.

Censorship at the destination, a routine hazard for the popular writer who chooses controversial topics, threatens the right even of scientists to publish their findings. Censorship of primary publications, such as Kinsey's volumes on sexual behavior, results in even less science information of substance reaching the public, if that is possible.

NOTE

1. It takes much information processing to review the literature on information flow. The reviewer would like to thank colleagues who supplied him with advice, reprints from their files, etc. -- particularly Pauline Atherton, Charles Bourne, Edwin Parker, and Wilbur Schramm. In spite of this excellent help it is possible that important studies have been overlooked, and it is not only possible but likely that factual errors and distortions have inadvertently been introduced into the text. Comment about the review, and about these points in particular, will be welcomed.

The reviewer would also like to thank Ann Peterson for typing and supervising the production of this review.

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